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PERISCOPIC LENSES.

Paper by Mr. W. A. Dixey, *Member.*

Read before the Optical Society on November 14th, 1904.

The subject which I have to present to you to-night is described by the term "periscopic," which is a word used to indicate lenses to which has been given a special form with a view to facilitating "looking about." In other words, we have to discuss how best to adjust the curves of a spectacle lens so that the marginal parts of the lens may be of the greatest service. The problem is a purely optical one, and resolves itself mainly into a question of the correction of aberrations. So that first we will examine what aberrations are pertinent, and what are non-pertinent.

With regard to chromatic aberration we are clearly helpless; as we are restricted to the use of a single lens. Similarly, the question of aplanatism does not affect us. The receiving screen of the eye is the macula, a minute disc, which always lies in the visual axis as the eye turns in different directions. The rest of the screen is curved, and as a matter of fact, nothing would be gained by exact definition on any part of the retina except the macula, so that for the purpose of our problem aplanatism may be ignored.

Spherical aberration requires more careful examination. We know that with a single lens the best effect in this respect is obtained by equalising between the two surfaces the angles of incidence and issue. Thus, for parallel light falling on a + 4.00 lens the surface towards the light should be + 3.43, and the other + .57, or in terms of the radii (taking $\mu = 1.5$) 146 and 877 mm., respectively. But it should be noted that if the light comes from a point situated $\frac{1}{4}$ of a meter in front of the lens (*i.e.*, an approximate reading distance) the best form for the lens is exactly the reverse of the previous one, viz., the shallow surface outwards and the more convex one next the face. And similarly at a distance of half a meter from the lens the best form is a simple symmetrical bi-convex. It is thus evident that

except for glasses that are to be used for objects at a definite distance, the defect of spherical aberration is a difficult one to allow for.

Fortunately, when we come to consider the amount of error caused by spherical aberration, we find it so insignificant that it may be neglected. Indeed, Dr. Percival, in a paper read before the British Medical Association in 1901, goes so far as to assert that the error is imperceptible. He gives as the radius of a foveal cone .001 mm., and he calculates the radius of the confusion circle for a — 20.00 plano concave lens, and he finds that in the least favourable position the radius is only .0003 mm. Obviously, says Dr. Percival, if the confusion circle on the retina is smaller than the sectional area of a foveal cone, this confusion circle will be perceived as a single point.

Dr. Percival takes 3.2 mm. as the diameter of the pupil, which is little enough as a maximum, especially when one considers the convergent nature of the light between the spectacle lens and the iris in Hypermetropia. Moreover, the question is not entirely one of the value of the radius of the confusion circle. However minute that may be, if it is taken as the measure of the difference of margin of any image at the focal plane, it is obvious that the inner margin of an image will frequently fall so near the edge of one cone-area that the outer margin will overlap on to another, and so will create at least a mathematical blur.

Later on, when we come to consider radial astigmatism, this will constitute a matter of some importance, but with reference to spherical aberration it is simply a question of definition, and without arriving at any decision as to whether the effect of the error reaches the limits of visual perception, we can agree on the strength of Dr. Percival's figures that spherical aberration may be neglected in determining the form of our spectacle lens. So that having disposed of chromatic, planatic, and spherical aberration, we are left with the one that really matters, viz., radial astigmatism.

To understand the effect of radial astigmatism, you must recognise that the eye ranges over the whole surface of the spectacle lenses. In an ordinary pair of oval lenses whose horizontal axis is $1\frac{1}{2}$ inches, you can cover an angle of 80°, or 40° on each side of the axial position.

At a distance of 10 inches, a column of print 3½ inches wide necessitates an oblique direction of the sight of 20°. So that it seems impossible that a spectacle wearer should avoid constant considerable obliquity of sight through his spectacle lenses.

Let us see what this means. A pencil of light passing obliquely through a lens acquires a quality which is most familiar to us under the term of astigmatism. Parallel light passing through a + 5.00 lens at an angle of 20°, laterally from the centre, enters the eye with the addition of a cyl. + .75 axis 90° effect. A wider angle gives an increased cylindricity, and even a lens of one or two diopters acquires in ordinary use a cylindrical effect of .25 or more. Now, in this case, it is not only the effect on the definition or the visual acuteness that has to be considered; it must be remembered that the greatest importance is attached to the correction of minute defects of astigmatism on pathological grounds, and if this is to be accepted it seems also of the highest importance that an artificial astigmatism should not be created by the form of the lens which is given to correct a simple spherical error, or that the effect of the spherocylindrical correction should not be neutralised or enhanced. How, then, are we to construct our lenses so as to obviate this error?

It was pointed out by Wollaston in the first half of last century, that if you place an eye in the middle of a hollow sphere, objects would be viewed perpendicularly through its surface in any direction. This formed a basis for the first suggestion for periscopic glasses. Taking as centre the centre of rotation of the eye, and as radius the distance from it of the spectacles, the glass should conform as nearly as possible to the imagined hollow sphere of Wollaston.

That is the first principle, which still governs the form of periscopic lenses. You will notice in passing that it is dependent on (*a*) the distance of the vertex of the cornea from the centre of rotation of the eye, and (*b*) the distance of the spectacle lenses from the vertex of the cornea. Two variable quantities which have to be taken as constants in the calculation of any table of values.

But the problem does not rest here. We have corrected the radial astigmatism due to the obliquity of

direction, and now we are met by another radial astigmatism which is due to the inherent obliquity of the two surfaces of the lens at any but a central position. With a concave lens this second factor modifies the first, and so goes to reduce the curvature of a periscopic lens. But with a convex lens the error is intensified, and there would seem to be no means of entirely eliminating it.

The exact calculation of these errors, and of their correction, so far as is possible by the relative curves of the two surfaces, makes a very pretty mathematical problem. The calculation was first made, I believe, by Dr. Ostwalt, a MS. translation of whose paper has been kindly lent to me by Mr. Oxbrow, of Norwich. It lies on the table for your reference. Dr. Percival has also made the same calculations, and has modified them, as I gather from the very brief account of his paper available, by his theory of the limits of permissible error in relation to the size of the retinal cones, to which I have already alluded. Both gentlemen give tables for the guidance of Opticians, and the tables vary considerably. Dr. Percival says that there is an error in Dr. Ostwalt's formula, and, of course, there is the probability of their having taken different values for their constants.

But however they differ in results, there is no doubt at all about the principle; and there would seem to be a clear case for the use of periscopic lenses, not only for the sake of better definition in the marginal parts of the lenses, but also on pathological grounds, because by their use we avoid the danger of creating an artificial astigmatism.

On the other hand certain objections have been urged against them, which may be classified as (1) optical, (2) financial, and (3) cosmetic.

Objectors on optical grounds generally fail to see correctly the exact conditions of the problem. They think, for instance, of the case for the correction of spherical aberration, but they forget that we have to deal not with the whole spectacle lens, but with a pencil of light never larger than is defined by the diameter of the pupil. I have already dealt with this, and have shown that spherical aberration is of no importance in this connection.

Increased expense does not appeal to me as a serious objection. Only in the case of the lowest-priced goods

need the cost of the spectacles be affected, so long as spherical curves are used. Sphero-cylinders can often be manipulated so as to give a considerable periscopic effect, and this is done without increase of cost. In some cases a toric curve is necessary, and that, under present circumstances, increases the cost of manufacture some four or five shillings, and so may legitimately affect the selling price. I think, however, that few people would object to pay a little extra for their spectacles if the doctor tells them that this form of lens is desirable.

A more serious objection is that of cosmetic effect, though, in this respect, perhaps our apprehensions are liable to be exaggerated. I do not think that a − 10. D plano on the face is essentially uglier than the corresponding bi-concave; or that a − 20. D bi-concave lens looks better than one worked with − 3. D on one side and − 17. D on the other, especially if the deeper surface be champfred. With shallow curves there is more to be said on this score. Dr. Percival's formula for − 2. D is + 5.25 D ⌒ − 7.25 D, Dr. Ostwalt's being + 2.50 D ⌒ − 4.50 D. Either glass would seem at first sight to be at a cosmetic disadvantage as compared with a − 2 D. bi-concave, but here again convention counts for much, and I doubt whether the flat lens is essentially more beautiful than the curved. Dr. Percival's weaker convex lenses are more objectionable in this respect; but it should not be forgotten, in fitting these lenses, that mathematical accuracy is not the simple end in view; the lens may always be modified by some limit of permissible error; and this would specially affect the curve of the weaker lenses.

Dr. Percival's limit seems to me unnecessarily stringent—I should like to see his .002 confusion circle in terms of the cylindrical power of the spectacle lens—a reasonable limit to work to would, in my opinion, be + .1. D. of astigmatism, and this would give much more useful shapes for the weaker convex lenses.

So much for objections; if there are others you will, no doubt, remind me of them, and I will endeavour to answer them in the course of discussion.

So that on the whole, after considering the case for and against, I have no hesitation in advocating a more general use of periscopic lenses than at present obtains. But they must be used with discretion and intelligence.

I have noticed a tendency in recent years to regard a table or formula found in a text-book as an entirely sufficient rule for general application in practice. As I have already hinted, such a use of Dr. Percival's table would be incorrect. It is based on average measurements, and is calculated to an unnecessary degree of mathematical accuracy. Such a table is invaluable as a guide, but we must know how to modify our lenses to meet the special dimensions and requirements of each case, and to avoid giving an unnatural form to our lens when nothing is gained but a mathematical accuracy, which is not required by the conditions of the particular case.

I hope that the chief point I have been able to impress upon you by these cursory remarks, is the importance of radial astigmatism in spectacle work. The use of periscopic lenses is one of the most valuable means at our disposal for meeting the difficulties created by this aberration; but it is not the only means, and I have with me here some specimens for your inspection, which are examples of other mechanical methods of meeting the same difficulty.

[Mr. Dixey then exhibited and demonstrated the use in relation to radial astigmatism of (1) an angled reading frame, (2) a pair of bi-focal spectacles with lenses set at an angle of 160°, (3) a long-handled lorgnette with hinged front.]

Discussion.

Mr. Dunscombe said he had been exceedingly interested in listening to Mr. Dixey, the more so because he supported the position which his (Mr. Dunscombe's) predecessor had taken up 60 years ago, when he introduced pantoscopic spectacles, which Mr. Dixey recommended for use in reading. He was especially interested in one form of spectacles shown where the lens for distance was placed perpendicularly and the reading lens obliquely, an arrangement designed by the late John Braham, and shown at the 1851 exhibition. The objection was that it did not look pretty, but he thought they must all be indebted to Mr. Dixey for the science he had brought to bear upon the matter. There was practically nothing to criticise. He had taught them all something, and especially the younger members. He

had shown them how to fit their customers, and described the best possible forms which would give the greatest satisfaction.

Mr. Purser said it had occurred to him that it would be very interesting if they could hear the opinions of those gentlemen who actually provided periscopic lenses, and to have from them the result of their customers' feelings. He himself had been told some time ago by a gentleman, that he did not find periscopic lenses so comfortable as bi-convex lenses.

Mr. Clarke said he had found that in practical use with periscopic lenses one never obtained anything like such good results from the double ground from 5.50 up to 8 diopters, as in bi-convex lenses. He would like Mr. Dixey to explain the reason of this.

Mr. Boatman was very interested in the last specimen —an improved lorgnette—shown by Mr. Dixey. He would like to know why it was that some persons who had been wearing periscopic lenses for some time were unable to wear bi-convex lenses with comfort afterwards.

Mr. Sutcliffe could not help thinking that a lot of the objections brought forward against periscopic lenses were simply bogies. With all due respect to Dr. Percival and Dr. Ostwalt, he was rather inclined to give in to the theory of Wollaston, that the mean curve of the periscopic lens should be approximately of a circumference of rotation from the centre of the eye. With regard to Mr. Boatman's question it had, he thought, suggested the reason why periscopic lenses were not always satisfactory. He had often thought that people who had been accustomed to wearing periscopic lenses were uncomfortable when put on to bi-convex spectacles, and *vice-versâ*. He (Mr. Sutcliffe) put that down purely to careless fitting. The periscopic lens was in a different position to the bi-convex. Spherical aberration, the speaker thought, was a comparatively trifling point. His own experiments with deep lenses had been made with apertures about the size of a threepenny bit. He had moved the disc with the hole in it in different positions before the lens, and found that the spherical aberration was comparatively very trifling. It had also been compared to the spherical aberration in a camera, but he did not think there was any comparison, because the camera did not tilt up and down. When they were

looking through the edge of a deep periscopic lens, to all intents and purposes the centre was blocked out, and he failed to see, therefore, why they should be troubled with spherical aberration. The trouble was, that they tried to have the same periscopic combination to every size and position of eyeball. They should consider whether a person had a small eye, and another thing they should consider was the distance the lashes were from the eye. His experience of the non-success of periscopic lenses was that they were put in a different position to that in which bi-convex lenses were put. If they had beep lenses on the Willaston principle, and not wobbly, they would be all right. They were apt to compare unfavourably deep periscopic lenses with the ordinary forms, but in good, perfectly worked, very deep periscopic lenses, and at correct distance from the centre to the eye they had an ideal lens.

Mr. Dixey, after thanking his audience for their kind remarks, observed that it was somewhat difficult to reply to Mr. Boatman's objection. He thought the best explanation was that put forward by Mr. Sutcliffe. So far as the conditions that he had been considering that evening were concerned, there was no theoretic difference between the performance of a periscopic and a bi-convex lens when the central part of the lens was looked through. There should be no practical difference, but if there were any it was to be acounted for by the fact that the bi-convex was better worked or polished. It was quite possible that a periscopic lens, in view of the greater depth of the curves, might not be so well polished. With regard to the objection that a periscopic lens was set farther from the eye than a bi-spherical, that would account for a certain want of definition, but that ought not to be, and it would only occur in the case where a customer brought a pair of spectacles and asked the Optician to substitute periscopic lenses for bi-convex. In that case a new frame was necessary, or the bridge should be altered so as to allow the nearer approach of the lens to the face. One of the small advantages of the periscopic lenses was that, when the lens was set at the right distance from the eye, the margin, or rim, of the frame occupied a less visible position than in the case of a bi-spherical. Mr. Dixey thanked Mr. Sutcliffe for his valuable criticisms, and observed that the suggestion

about Wollaston's periscopic was instructive. He did not think, however, that Mr. Sutcliffe saw a point which was, in fact, the main point that was involved in the mathematical consideration of these lenses, and it was that, in addition to the obliquity which was caused by the eccentric direction of the sight, they had an intrinsic obliquity caused by the inclination of the two sides to each other. Mr. Purser had remarked that unsatisfactory results were sometimes observed by the patient; this, of course, might be due to faulty fitting.

Mr. Hyatt-Woolf, in moving the vote of thanks to Mr. Dixey for his interesting and useful paper, said he would like to give his own experience of periscopic lenses. When speaking previously during the evening, and until Mr. Dixey had held him up as an object lesson, he was quite unconscious that he was wearing glasses. That was one great advantage of periscopic lenses, the wearer gets them closer to the face, with the result that he is unconscious of their presence, because he does not see the objectionable rims which one notices in glasses of the usual form. An additional advantage derived from lenses of this kind was that one obtained a wider field of vision, and they were far more restful to the sensitive eye.

Mr. Marks, in seconding the vote of thanks, stated that they all looked upon Mr. Dixey as a father to them in matters of the kind they had just dealt with. The subject he had introduced to them that evening was one which would prove most valuable to them in the future.

DIRECT STEREOSCOPIC PROJECTION.

Paper by Mr. Theodore Brown.

Read before the Optical Society, December 15th, 1904.

The opportunity now afforded me of bringing before your notice some of the results of my recent experiments I esteem an honour and one of great responsibility. The kindness you have extended to an outsider, such as myself, gives me confidence in anticipating the unanimous sympathy of this Society; and my only anxiety now is, that the pictures projected may come fully up to your expectations.

We shall look at pictures that have been produced at various stages during the development of this new system, and I ask for your kind allowances for varying imperfections, in the earlier productions. Euclid, and other philosophers of old, observed the dissimilarity of the two retinal images in binocular vision, but they do not appear to have conceived the idea of reproducing such phases for inspection in a stereoscope. Thanks, however, to the researches and ingenuity of such men as Elliott, Wheatstone, Brewster, and others, we now have this instrument, the means whereby the pleasing sensations of natural phenomena, may be imitated; nay, copied with such precision, that the keenest intelligence, the most alert observer, is deluded by so perfect illusions.

But despite the existence of this ready means for demonstrating the true nature of stereoscopic effects, misconceptions still persist as to what actually constitutes the term involved. Hence it will be as well for us to refresh our minds with the qualification demanded.

We are told that the word "stereoscope" is derived from two Greek words, signifying "solid" and "to see." So that we understand that to see an object stereoscopically, that object should be perceived not merely in relief of its surroundings, but also invested with the still more important property of "solidity." Any subject rendered with this two-fold qualification can truly be termed "stereoscopic."

This effect, which arises in the process of binocular perception, is also appreciated in dissimilar pictures in the stereoscope; but the limitations of that instrument have induced experimenters to look for some system of lantern projections that will enable a large audience to see the results simultaneously. Those of you who are acquainted with the excellent series of articles on this subject, contributed to the 'Magic Lantern Journal,' by Mr. Thomas Bolas, will remember that all the systems therein described depended for their final results on some sort of intermediate aids to be used by each person in the audi-

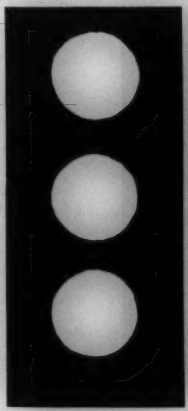


Fig. 1.

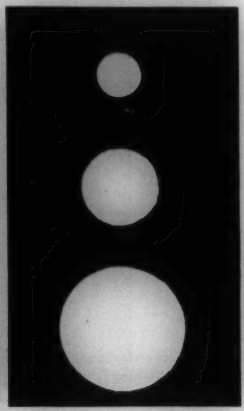


Fig. 2.

torium, such aids being composed either of coloured glasses, revolving shutters, refracting, reflecting, or polarising media.

These necessities in the old systems were obvious drawbacks to success, and it is, therefore, with a certain degree of satisfaction that I am able to bring before your notice this evening, a system in which no intermediate aids are required; trusting that the degree of success the invention has as yet attained may be supplemented and strengthened by your kind approval.

Before we proceed to consider how stereoscopic effects are to be "directly" projected upon a lantern screen, I wish to draw your attention to some of the conditions of light and shade, that are calculated to suggest distance and solidity, which, however, are not stereoscopic effects. If we place three discs of white paper, fig. 1, equal in size and one above the other, there will be absolutely no illusion of relief, they will all appear to be situated at one distance from the eye.

FIG. 3.

To perceive the illusion, hold this diagram at arm's length, and view with one eye only.

If, however, we vary the relative sizes of such discs, in the manner shown in fig. 2, we shall embrace the laws of proportion and perspective, and by so doing create in the observer's mind an idea that the smallest disc is the one farthest from the eye; the middle-sized disc at the intermediate plane, and the largest disc in the immediate foreground. But the illusion is weak, being somewhat unnatural; for if in nature we place three discs of equal size at varying distances from the observer, a close analysis of the retinal images will show that they

not only vary in size, according to their respective distances, but that they will also vary as regards tone or luminosity.

Thus nature gives us the hint, that to suggest dissimilarity of distance, the largest of the three discs should be made the most brilliant, the middle size one less so, and the smallest one of the weakest tone. We will, therefore, take this hint and note the effect obtained (see fig. 3).

Fig. 4.

I think you will agree with me that it requires no extravagant imagination to enable us to appreciate the three discs here presented, as being located at different distances from the eye. This, then, is an example of an arrangement of light and shade capable of creating an illusion of relief, similar, yet not identical, with stereoscopic effect. We may now turn our attention to a second condition of light and shade which, in this instance, will

suggest solidity, namely, *local gradation of tone*, by which we mean the shading up of each individual object in a collection of objects. If we make a drawing of a group of models, such, for instance, as a cube—a vase—and a ball; the drawing may be perfect as regards its proportions and lineal perspective, but the absence of light and shade robs it of its power to delude.

Immediately we add this additional criterion, our drawing at once assumes solidity, and to the casual observer the subject may appear as real objects in space.

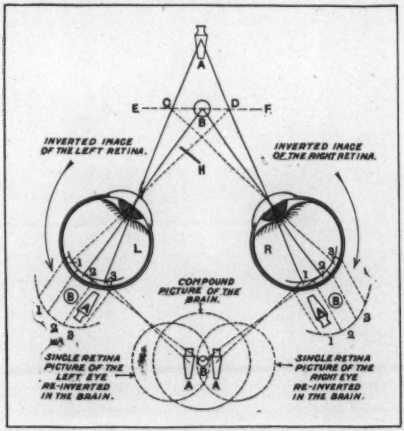


FIG. 5.

Subjects that lend themselves to this illusion are those that are already known to be solid, such as statuary, vases, etc., an example of which is shown in fig. 4. But this illusion also ceases to deceive under mathematical analysis.

We have looked at two conditions of light and shade, the first one capable of suggesting distance, whilst the second suggests solidity. A combination of the two, giving to us both solidity and relief, must not be confused

with the solidity and relief of the stereoscope. We shall omit to mention the conditions of colour, insomuch that we know the stereoscope to depend on no such agencies for its peculiar results. Doubtless, we are all, more or less, acquainted with the causes of apparent solidity obtainable in the stereoscope; but it is not enough for us to know that because nature paints upon the two retinæ of the eyes images that are not precisely alike, and that, therefore, it is necessary that our pictures in the stereoscope should be correspondingly dissimilar. No! if we wish to strengthen our views on this subject it will be necessary for us to look deeper into the physical provisions of nature, and in practice to anticipate, as far as possible, the requirements of her laws.

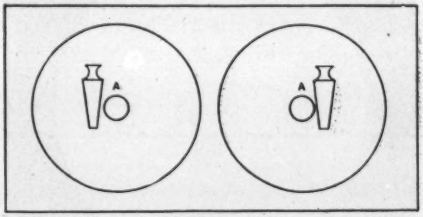


FIG. 6.

Let us, then, turn our attention to the diagram or section of the human eyes (fig. 5). The first fact to be observed is that there are corresponding parts in the two retinæ. Such parts are really microscopically minute, but for the sake of simplicity we shall suppose the little divisions marked 1, 2, and 3, to represent the parts referred to. When light emanates from any object and strikes corresponding parts in the two eyes, there will be a single impression produced in the mind, though that impression has a duplicate origin. Thus light emanating from the object B, which falls on part 2 in each eye, gives rise to a single image in the mind.

The axes of the eyes, an imaginary line drawn through the centre of the pupil to the retina, is always directed to the object of immediate attention, and will meet therein. Any object such as D, situated in the same

plane with the object of immediate attention, will also be projected upon corresponding parts of the two retinæ 1—1, and will also give rise to a single impression in the mind. An object such as the vase A, removed from the plane of immediate attention, will give rise to a double impression in the mind, two images of the single object being noticable.

This duplication of images is due to the light striking *contrary* parts of the two retinæ, 3 in the left eye, 1 in

FIG. 7.

the right. Perhaps this phenomenon is more clearly demonstrated (but in a reverse order) if we hold a single finger before the face and then gaze at a remote object. Two fingers are seen, one on either side of the distant object. If, now, we turn the attention away from the distant object, and centre the axes upon the near finger a single impression from the latter will arise, whilst the remote object, that hitherto appeared single, will now be duplicated.

This compound effect in the mind, when looking at a

ball and vase, is represented within the large dotted circle of our diagram, whilst the compositions within the two smaller circles represent the respective images of the retinæ. If we placed photographic cameras in the positions of the eyes, the dissimilar pictures of the vase and ball will be as indicated within the small dotted circles, and when mounted for the stereoscope would be as shown in fig. 6.

FIG. 8.

On examining these pictures in the stereoscope, it is possible to blend either the pictures of the ball or of the vase into one, but not both these objects may be seen singly at one adjustment of the axes. If we turn the attention particularly to the ball, we shall see a dim image of a vase on either side of it; and if we look more especially at the vase, then a single vase will be seen with

a dim image of a ball on either side of it, as shown in our next diagram (fig. 7).

From these considerations it is evident that in binocular vision no two planes can be absolutely superimposed at one moment, the cause being due to the dissimilarity of the retinal images obtained from separate view points. These facts are so vital to the remarks I have to make later, that I must venture to illustrate the truth in another way (fig. 8). Suppose we make a pair of transparencies from negatives obtained in a stereoscopic camera, these may be taken to represent the dissimilar images in binocular vision. If we superimpose the nearest object, *i.e.*, the white flower, we shall find it impossible to make the dissimilar images of the remote plane coincide. Hence, two images of the man in the background are seen, as in fig. 8. If we superimpose the remote images, then foreground objects will be duplicated.

Thus we find that the full and complete appreciation of a landscape involves a constant variation in the direction of the eyes' axes; and we may add, a constant readjustment of their refractive humours. These facts should suffice to dismiss from our minds the slightest entertainment of the erroneous idea that a single picture can, under any circumstances, produce stereoscopic effects. We have already stated that the limitations of the hand stereoscope induced experimenters to seek for the means whereby stereoscopic effects could be shown on the lantern screen. A review of all the methods devised would show that each inventor in his turn assumed that the only way of creating the desired result was to present the dissimilar pictures of the stereogram to the respective retinæ, so that each eye saw its proper element.

This assumption, which we shall endeavour to dispel, brought each inventor face to face with the awkward fact that the conditions of binocular vision made it necessary that each eye must not only see its proper element, but also that each must *not* see the element belonging to its companion. Under conditions of binocular vision, therefore, intermediate aids became the necessary nuisances. As far as scientific research has yet arrived, no system has been suggested that dispenses with the need of intermediate aids, and at the same time complies with the laws of binocular perception.

Seeing the hopelessness of ever solving the problem under binocular conditions, we were led to ask ourselves the question, "Is it not possible, in virtue of the sympathetic nature of the two retinæ, to transmit through a single eye mental impressions not differing very substantially from those gained during the use of both eyes?" In turning our attention to this new phase of the subject, we did not know at the time that we were about to propound a new theory, and do somewhat towards the development of a unique invention, namely, "Monocular Stereoscopy."

Many of the optical illusions to which our eyes are subject may be explained by the circumstance that the retentive power of the memory is superior, as regards duration of impression, to that of the retinæ of the eyes; and to this inequality of physical and mental persistence we attribute the possibility of "direct stereoscopic projection," or as we have otherwise termed it, "monocular stereoscopy." We have the united support of eminent philosophers, together with practical and ready proofs, that an image projected upon the retina persists but only $\frac{1}{8}$th part of a second, whilst numerous examples could be supplied to show that such an impression will be retained in the mind for a very much longer period of time. It seems to have escaped the notice of all experimenters, that in virtue of the fact that there are corresponding parts in the two retinæ, we may use both or only one set of nerves for the transmission of pulsations to the brain; the result in the mind, under both circumstances, being substantially the same, though not minutely identical.

Referring again to the sectional diagram of the binocular system (fig. 5), let 1, 2, 3, in each eye represent the corresponding parts referred to. If we place two vases on the line E—F, at points C and D, respectively, cancelling the vase A, the image in the mind will be similar to that shown within the large dotted circle. This is with the axes still centred at B, as shown. What will happen if a shutter H is allowed to intercept the rays from D to the left eye L? Will any noticeable change take place in the compound image of the brain?

The answer is in the negative, because, although point 1 in the eye L is not stimulated as before, its corresponding point 1 in the companion eye R *is* stimulated, being uninterrupted by the shutter H. Hence whilst the vase on

the left in the compound image (within the larger dotted circle) will be transmitted thereto by companion nerves from point 3—and, therefore, will be a compound or binocular impression—the vase on the right within the large dotted circle will be a simple or monocular impression, because it is transmitted thereto only through one set of nerves, emanating from but one point, 1 in the right eye.

But whilst this is the case there will not have been any noticeable change in the mind by the interposition of the shutter H—the observer would not be conscious of any reduction of stimulus, unless his attention was particularly directed to the fact; and even then it would be with difficulty that he could determine from which eye the light had been excluded.

These facts seem to justify the conclusion that it is possible to fully convey to the mind the nature of a flat sur-

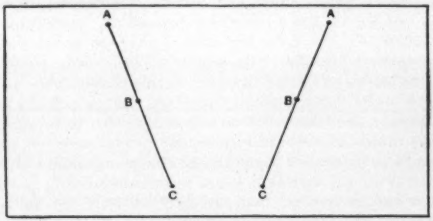


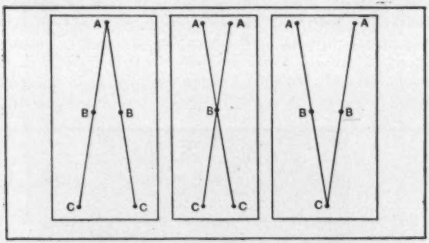
FIG. 9.

face, through both or only one set of the optic nerves, without substantial differences in the total effect produced, so that we are led to ask the question, "May we not go further and by some means transmit the sensations of depth, also through both or a single eye?" Before we proceed to show how such results are to be brought about, we must observe that, as binocular vision consists more or less in an incessant modification in convergence and divergence of the axes to various planes, there must be a lateral displacement upon the retina of images of external objects as different ones are successively observed.

Thus, referring to figure 9, which represents the two elements of a stereoscopic picture of a long bar with

knobs, when superimposed by the stereoscope, we may look successively at points A, B, C. Looking fixedly at A, we shall notice by oblique rays that C, C, do not coincide, and the impression in the mind will be similar to that shown at fig. 10. Sliding the axes along the bar and looking fixedly at C, the image in the mind will then be as 12, whilst if we look fixedly at B, we shall observe the effect shown at 11, *i.e.*, a **X.**

As we have already intimated, the sensation of relief in binocular vision arises during the active progress of the axes from one plane to another, and now we observe further, that the images of objects situated at varying distances from the observer traverse the retina laterally, crossing and recrossing the axes, as the latter concen-



FIGS. 10. 11. 12.

trates its attention upon various planes. Thus in looking from A to C (fig. 9 combined in the stereoscope) A first occupies the axial centre of the eyes, and then, as the attention is turned to C, A becomes doubled, and moves to the right and left of the axes simultaneously. This circumstance gives us the hint that if we oscillate to certain degrees different parts of a picture within a given time and in a proper manner, we shall at once create an artificial axial accommodation.

In our next diagram, fig. 13, we have the binocular or stereoscopic phases of a cone with its base farthest from the eyes, but these two figures show only the *extreme* phases, the most central intermediate phase being as indicated by figure 14. If, now, we present to the eyes or eye, not merely extreme phases (fig. 13), and a central

phase (fig. 14), but also a large number of intermediate phases coming between these, we shall then create in the mind a sensation of relief. A primitive method of carrying this experiment into practice is by using a disc (fig. 15).

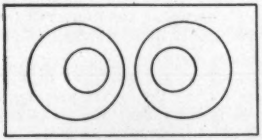


FIG. 13.

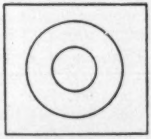


FIG. 14.

On close examination it will be found that the inner circle of 32 diagrams represents the dissimilar phases of a cone. On revolving the disc on its axis, and in front of a mirror, we may look through the slits from behind, thus viewing the successive diagrams by reflection in the mirror; this will give an impression of all the diagrams combined, resulting in an image in stereoscopic relief.

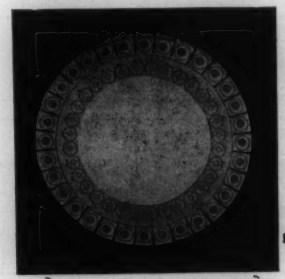


FIG. 15.

We thus demonstrate, though somewhat imperfectly, *persistence of vision* and *mental retention*, the apparent continuity of physical impression having to do with the first law, and the effects of relief with the second.

But to carry the same experiment to greater perfection we require photographic images. For this purpose we

arrange a group of articles and take a number of phases by means of a camera. These pictures we arrange on a similar disc, fig. 16. On inspecting them in the same manner as we did the cone, we have the additional quality of tone to aid the senses, with the result that each article is naturally allotted its respective plane, whilst there is no danger of pseudoscopic effects, as was the case with the diagramical series of pictures of the cone.

But whilst this experiment proved more effective than the last, so much light is lost by the opaque parts between

FIG. 16.

the slits that we found the series of pictures could be better seen when applied to an apparatus similar to the praxinoscope (fig. 17). In this instrument the illumination of the subject is continuous. The principle of monocular stereoscopic projection, which we hope is now made clear, naturally suggested calling into use different types of kinematograph cameras, whether for the production of stationary or animated subjects. For the latter it is absolutely necessary that such cameras should be used, and that the bioscope or kinematograph

projector should be employed for their exhibition upon a lantern screen.

Time will not permit us to dwell upon the possibilities of adapting monocular stereoscopy to the ordinary lantern. We first used the kinematograph camera in the manner indicated in fig. 18. A supplementary piece of mechanism was used, capable of operating the camera and at the same time, by means of a connecting rod, of oscillating a shelf or table upon which the subject was arranged. We will now look at a picture so obtained.

PROJECTION OF PICTURES BY MEANS OF A BIOSCOPE.—You will notice that whilst there is a pronounced stereoscopic effect produced, the results are somewhat discounted by the very apparent oscillation of the foreground objects. This movement is due to the fact that sufficient intermediate phases of the subject were not included; in other words, the number of dissimilar pictures contained in the complete cycle were too few. Recognising this, we proceed to increase the number of pictures in the cycle; and by so doing the oscillation is correspondingly reduced; and although there is a slight reduction in the stereoscopic effect, the result obtained is more agreeable to the observer.

FIG. 17.

PROJECTION OF PICTURE BY BIOSCOPE, ILLUSTRATIVE OF THE LAST REMARKS.—It should be mentioned that the oscillation may be reduced in several ways; we may mention two here, viz., by enlarging the number of pictures in the cycle, or by decreasing the distance of displacement of the subject.

But the apparatus with which the picture of the vases was obtained could only be used for inanimate subjects, or for such small subjects as would permit of being placed upon a table; so that for animated subjects of any kind whatsoever, we devised the supplementary mechanism now shown upon the screen (fig. 19). On

turning the handle, the camera is operated, and by means of a crank (not visible in the picture) the camera was recroprocated whilst resting upon a bed, and sliding on parallel bars. The displacement of the camera could be regulated to suit the subject in hand. The series of pictures obtained were not merely dissimilar as regards successive phases of motion, as in an ordinary kinematograph film, but also dissimilar along a horizontal plane at right angles to the subject. We will first look at some

Fig. 18.

still-life subjects, taken by means of this apparatus. [Demonstrations with bioscope followed.]

Now, it was found that whilst the camera was allowed to oscillate, parallel to the subject, or with its axis at right angles to the subject, the side-swing of foreground objects proved somewhat disagreeable to the eye. To avoid this as much as possible concentricity of the axis of the lens was adopted. Thus, in reference to the figure upon the screen (fig. 20), as the camera (marked C) was

made to oscillate, the parallel bars upon which the camera bed recroprocated were curved, so that the axis of the lens concentrated upon a suitable or middle plane of the subject, radiating to a point A. By this means the oscillation of objects was distributed throughout the entire depth of the subject; thus dismissing to a very large extent all objectionable movements, as will be seen from the next two series of pictures shown. [Bioscope projections followed.]

Our next series is illustrative of Farm Scenes—commencing with the cows being driven home to the farm,

FIG. 19.

cows being milked, and finally, the process of making the butter. For these pictures, which were taken by the special stereoscopic apparatus already described, we are indebted to Mr. R. W. Paul, who is doubtless well known to most of you; and it is gratifying to be able to say that we have the co-operation of that gentleman in the work of producing stereoscopic living pictures. Mr. Paul is making extensive preparations to supply the market, and in view of the fact that, since these pictures were taken, improvements have been made in the patent apparatus, we can promise even better results in the future

than as yet have been obtained. We will now proceed to take a peep into the country. [Bioscope projections again followed.]

The next picture we have to show (fig. 21) is of the still-life type. Here the axis of the lens radiated to a point six feet distant from the camera, and as the lady shown occupied the radiant point, you will note that she is absolutely stationary. [Bioscope projections of fig. 21 were then given.] It has been suggested by your worthy Vice-President, Mr. Charles Hyatt-Woolf, that to give a fair opportunity for judging the differences between these stereoscopic pictures and the ordinary cinematograph productions, the ordinary pictures should be shown, immediately followed with stereoscopic ones, and for this purpose we have taken the same subject, under both systems, and made one continuous band of the two. We do not know which will happen to be projected first, and we shall leave the audience to decide for themselves the distinction. [Bioscope projections followed.]

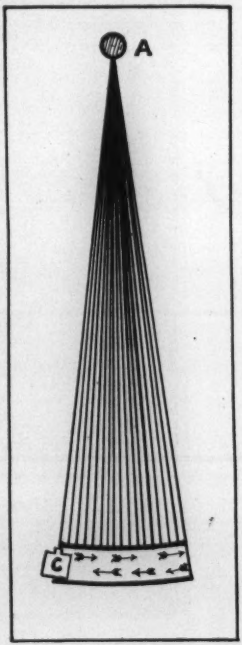


FIG. 20.

For the next series of pictures we have to thank Mr. Collier, of Messrs. Pathé Freres, who has been kind enough to allow us to include this subject in our programme to-night. Before looking at fig. 22, we may refer for a moment to the diagram upon the screen (fig. 23). If we use only one eye when looking

from a railway carriage window in motion, that eye will be carried in panoramic order, as indicated by the successive dotted circles. In viewing this external landscape, composed of objects at A, A 1, B, etc., let us sup-

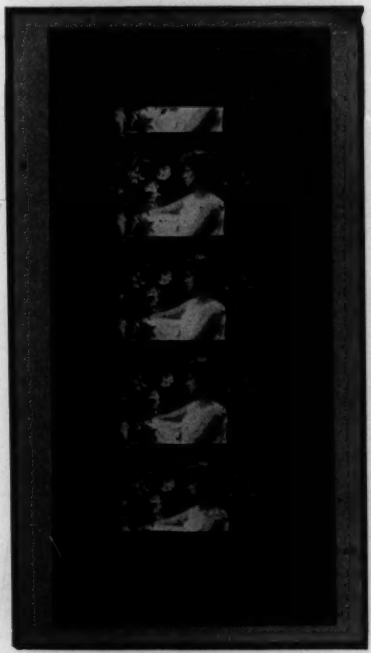


FIG. 21.

pose the attention to be directed to the remote object A. Whilst watching this, the eye, carried from C to D, will turn in its socket, with the motion of the train, so that the image of A, though viewed from successive positions, remains stationary upon the retina.

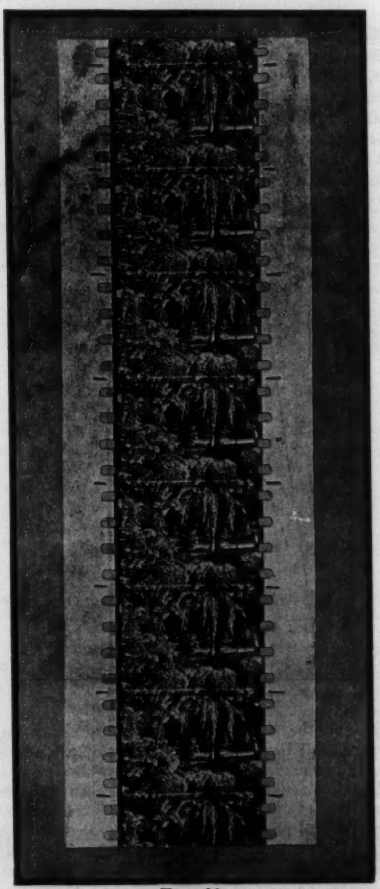


Fig. 22.

When it has reached D, by reason of inconvenience to continue its gaze, through the altered position of the train, it will suddenly turn to another object, A 1. This constant re-adjustment of the eye can be seen by looking at our companion on the opposite seat in the train, as he or she watches external things. The result is stereoscopic effect in the mind, which may be reproduced by placing a camera in the successive positions, C, D, E, etc., and photographing the landscape.

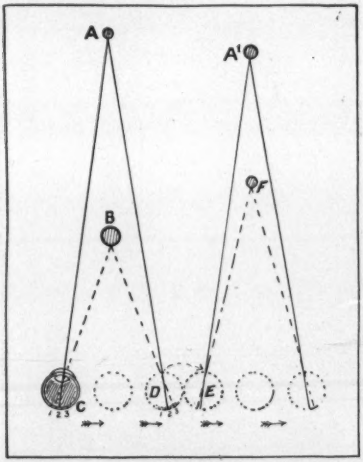


FIG. 23.

This may be done by means of a cinematograph camera operated upon any moving vehicle, and when a positive film, made from a negative so obtained, is projected in the usual manner upon a screen, stereoscopic effects will be the result. It is evident, however, that this panoramic and progressive movement cannot be applied to all subjects without interruption at short intervals; so that what we are endeavouring to do is to recroprocate the camera, or subject, in such a way that an equal

stereoscopic effect may be retained, and this without pronounced oscillation of various parts. I think we may say that the pictures we have been looking at this evening justify the conclusion that "Direct Stereoscopic Projection" is no longer the dream of inventors, but a tangible and present reality.

The Discussion.

Dr. Walmsley (Chairman), in inviting questions or discussion on the paper, said it was rather a difficult thing to speak on the spur of the moment on a matter like the present, because there was a good deal of theory about it, but a starting point was furnished in the last film, because all had noticed how an increased section of solidity of a distant object was obtained in looking out of a window when travelling in a train. The landscape seemed to settle into different planes, as if cut out of cardboard, as the angle of position was changed the planes of landscape were seen to separate themselves from one another, and so a section of solidity was obtained. He had never seen such a good effect as that just produced on the screen, and did not know how far it bore on the full theory of stereoscopic vision, because, of course, he still held that with bodies under ordinary circumstances, it was largely the convergence that gave the section of solidity.

Mr. Hopwood said that in some of the views he could not help noticing, not the solidity but the oscillation. In some of the slides put on the screen at the commencement the oscillation was very marked. As soon as he got his eye on to a straight line, however, such as one of the bridges, he seemed to lose that feeling. Of course, some people could not see a stereoscopic view at all. The last picture was incomparably stronger than any of the others. In some of the others, especially those with bridges, he caught the impression of solidity—where isolated objects were standing at different distances.

Mr. Chalmers said he thought the pictures showed very much better when there was no object moving across them in the foreground. When the object was moving towards the camera the stereoscopic relief seemed to be very much better; but objects crossing the picture tended to efface the stereoscopic relief. He should think those pictures in which there was a movement of some

body across the picture would be very much more difficult to take unless there was a regular and continuous motion. He did not know whether any serious difficulties had been encountered in giving the views under those circumstances, and he would like to hear what Mr. Brown had to say as to this.

Dr. Walmsley said that before asking them to record a vote of thanks to Mr. Brown for his paper he might be allowed to make a further remark or two. He was sure they had all been very delighted with the simple and elegant way in which Mr. Brown led up to his theory and illustrated it with various lantern slides. He had thoroughly enjoyed this, because it was part of the business of his life to break down complicated subjects and present them simply to students, and when he saw this well done he had great admiration for the man who did it. He agreed with Mr. Hopwood that the finest study of stereoscopic effect was obtained in the last slide. No doubt the earlier ones were those in which the inventor was feeling his way. Then, again, the final slide was taken under very different conditions indeed from any of the others. As Mr. Hopwood remarked, everyone had not the faculty for getting stereoscopic impressions, and he (Dr. Walmsley) had noticed that the stereoscopic impression would come into his mind and then disappear again. The oscillation was painfully perceptible, but by fixing the attention to about the spot where the fixed point was in the picture, that seemed to disappear, and you got the relief at once. It would therefore appear that in presenting these pictures to a promiscuous audience it would be an advantage if a few directions were given how to look at the pictures, as if the attention was fixed on the foreground entirely the stereoscopic effect was not obtained. Dr. Walmsley, in concluding, said he would ask those present to accord to Mr. Theodore Brown a very hearty vote of thanks for the delightful entertainment he had given them, for the admirable paper, and for the beautiful illustrations accompanying it. (Applause.)

Mr. Brown, in responding to the vote of thanks, said that had he noticed Mr. Hopwood was sitting so near to the screen he would have asked him to go to the rear of the lecture-room, as Mr. Hopwood might then have escaped some of the unpleasant effects he mentioned, and

this justified the Chairman's remarks, when he said that it required some little instruction how to view such pictures properly. In looking at a stereoscopic view, the angle extended from the axis of the eyes was considerable, but the magnitude of the pictures shown on the screen, in relation to the point of the first disc, was really an exaggeration of that angle; and for this reason Mr. Hopwood had no doubt missed some of the effects. In reference to his remarks about the panoramic view being the most effective—and the Chairman had also made the same remark—the reason was that the images were projected upon the retina, and the sensation was carried to the brain in progression. There was no reciprocatory movement. That was the great difficulty in applying this stereoscopic system to such subjects—that when the recurrent action took place there was a slight interruption of the general effect produced.

One gentleman made a remark about the crossing of foreground objects. Really, that should produce the best effects. The effects produced when objects were coming towards us and increasing in magnitude were not stereoscopic. In that celebrated film, "A Trip to the Moon," one appeared to approach the moon as the image was made to enlarge on the screen, and there was a sensation of relief, but that was very different to what they saw in the stereoscope, and what was being aimed at under the system he (the lecturer) had been illustrating. He did not think he had anything else to reply to, and he would like to thank them for their kind vote of thanks, and to add that it had been a pleasure to him to show some results of his experiments.

A METHOD OF TESTING PRISMS.

PAPER BY MR. S. D. CHALMERS, M.A., *Member*, and MR. H. S. RYLAND, *Student Member*.

Read by Mr. Chalmers before the Optical Society, January 16th, 1905.

RIGHT-ANGLED prisms form a very important part of many optical instruments, and, in many cases, it is desirable to measure the angle with considerable accuracy. When the three faces are plane and polished the right angle can be readily tested with very great accuracy with the aid of an auto-collimating telescope.

To test the right angle a beam of parallel light is allowed to fall on the hypotenuse face B C (fig. 1), where it is refracted; one portion is reflected by the face A C, and, subsequently, by the face A B to the face B C, thus returning almost in its own direction, while the other portion is reflected by the face A B and the face A C.

If the right angle be perfect the two beams will return parallel to each other and to their original direction—even if the original light be not normally incident—provided only that the edges of the prism be at right angles to the direction of the original beam.

If, however, the angle differ from a right angle, the two beams will be deflected from the original direction in opposite ways by an amount, the difference being measured while the beams are still in the glass, equal to twice the error—a—in the right angle. Thus, in the glass the two beams will be inclined at an angle $4\ a$; on emergence into air this inclination will be increased by an amount depending on the refractive index μ of the glass and the angles of incidence; when the incidence is nearly normal the final angle will be $4 \times \mu\ a$.

If a small slit be placed at the focus of a well-corrected telescope objective, light from any one point will emerge as a parallel beam, will be reflected by the prism and form two parallel images of the slit in the focal plane

of the objective, and the distance d between corresponding points will determine the error of the prism, this error being $\frac{d}{f} \times \frac{1}{4\mu}$, where f is the focal length of the objective. If the eye-piece be fitted with a filar micro-

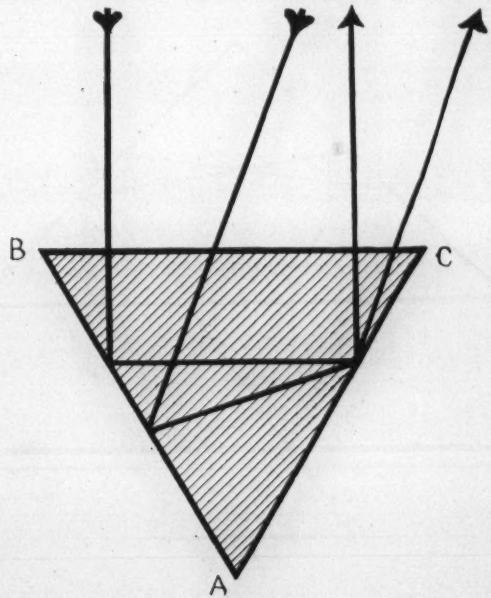


FIG. 1.

meter, or a ruled micrometer, the error in the angle of the prism can be read off with an accuracy depending only on the perfection of the surfaces and the telescope.

When the prism is rotated to bring the surface B C very nearly normal to the axis of the telescope three

additional images will appear in the field, and these should be exactly of the same height in the field. Of these three images the central one is formed by direct reflection at the front face, while the other two are formed by two reflections from each of the faces A B, A C, and one at B C. As four of these reflections are total the images will be sufficiently bright to observe.

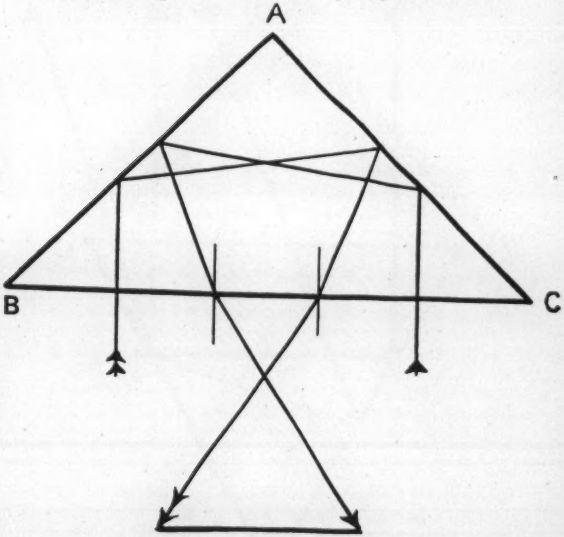


FIG. 2.

If these three images are of the same height, the faces of the prism are strictly parallel, and the amount of departure from parallelism can be determined by measuring the difference in height of corresponding points. For this purpose and for greater accuracy in focusing, the slit is provided with a cross line.

MEASUREMENT OF OTHER SPECIAL ANGLES.—The same method and apparatus can be applied to the measure-

ment of angles in a number of special cases. Fig. 3 represents the method of testing the 45° angles in the case of a right-angled prism, the two images to be observed are first (1) by direct reflection at the face A B, (2) by normal reflection at the face A C, after total reflection at B C, the final observed angle is 2 μ (B—C).

In fig. 4 the case of a 60° prism is shown; the equality of any pair of angles is readily tested, the difference

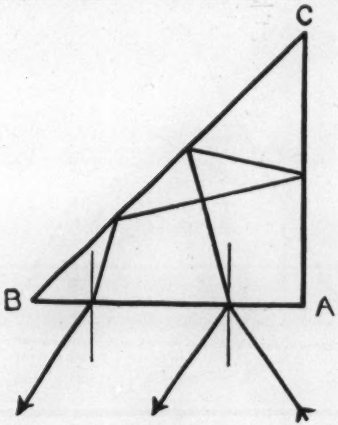


FIG. 3.

between B and C by the angle between the two images formed by reflections at A B and A C, and A C and A B, respectively, while each of these angles is compared to A by the aid of the direct reflection at B C.

Fig. 5 represents the methods of examining the special case of prisms of 30°, 60°, 90°. Fig 6 shows the method of testing the equality of the angles of an isosceles triangle where the equal angles are less than 45°.

PLANE PARALLEL GLASS.—A similar method has been used for the testing of plane parallel surfaces. Fig. 7 shows the method of measurement used in this special case; the angle measured is 2 μ × angular error.

The only difficulties of the method are those introduced by the curvatures of the faces. If the faces to be tested are curved it is obviously only possible to determine the average angle between the faces, and within the variations produced by the curvatures the measurements can be readily made. When, however, the hypotenuse face is slightly curved it is necessary, in order

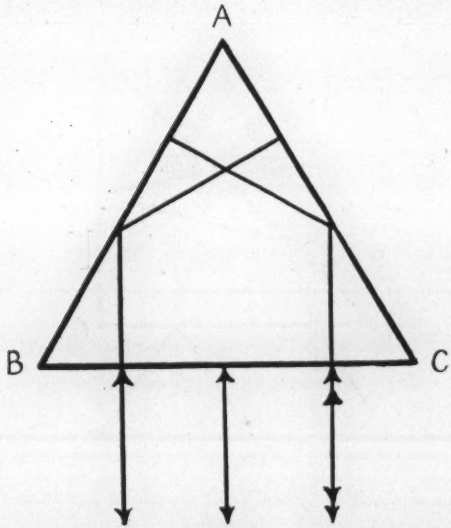


FIG. 4.

to obtain the necessary definition for reading the images, to re-focus.

In so doing it is desirable to obtain approximately normal incidence, and then the curved surface acts as an additional lens forming part of the auto-collimating device. It is then necessary to re-focus the objective, and this is done most easily by first setting the objective at the determined focus for parallel light, and slowly

departing from this position till the images are sufficiently sharp to read. This is much facilitated by using a fine line across the slit so that its image, as seen in the two faces, is, necessarily, continuous.

By this method the focusing can be done with very great accuracy, and the right angle can be tested to 1″ of angle, provided the glass is sufficiently homogenous;

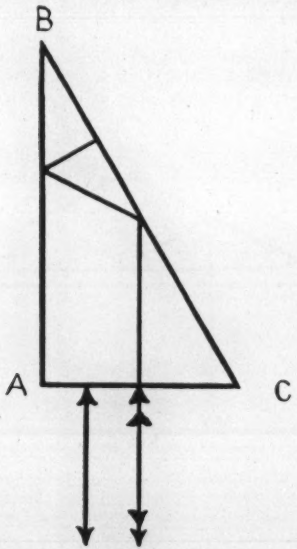


FIG. 5.

while in the case of plane parallel glass the error will not exceed 2″, and, if there be no appreciable curvature on the surfaces, when tested with a test plate, it will not exceed 1″. The method is, however, susceptible of very much greater accuracy when such is desired. For this purpose the slit is replaced by a small circular aperture, or, better still, the image of a small circular aper-

ture and the final measurements are made from the apparent ellipticity of the image or its diffraction rings. By this means the accuracy will be limited only by the want of homogeneity of the material of the prism, provided a sodium flame be used as the illuminant.

The instrument consists in its present form of a firm base plate upon which is mounted, horizontally, a telescope at a height, from base to axis, of about 10 cm. This telescope has a well-corrected objective of about 35 mm. aperture mounted in a sliding tube provided with rack focusing motion. The body tube is bored to a standard size so that various eye-pieces may be used for different purposes. The chief requirement of the auto-collimating eye-piece is that it shall have an ad-

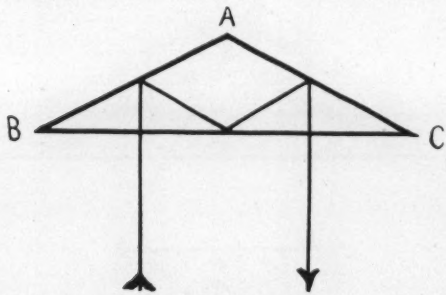


FIG. 6.

justable slit (or other means of obtaining a small source of light or image of such source) and crosswires, and an illuminating device such as a small window and a right angled prism. Opposite this window is mounted, on the base plate, an adjustable clip with lens and reflector to carry a sodium flame, Geissler tube, or small lamp for the purpose of illuminating the slit.

In front of the objective is mounted firmly a vertical pillar, adjustable for height by rack and pinion and a clamp screw. At the upper end is fixed a plate about 10 cm. in diameter, having a tapered pin in the centre. About this pin another plate of the same diameter rotates. A clamp and tangent screw is provided between

these plates for fine adjustment, and the edges are divided to degrees and half degrees. Mounts, clips, or stands to carry any form of prism or plate are placed on the upper plate. The vertical height is adjusted by the rack before mentioned, and tilt by three levelling screws on the prism holders or clips.

To make the measurements the instrument is levelled up (sight holes being provided in the body of the telescope to assist in this operation), the prism to be tested is placed on the stand in front of the objective and adjusted for height and tilt. Then by means of the auto-collimating and illuminating device an image of the slit is thrown on the prism and the coincidence, or amount of departure from coincidence, measured by the cross-wires, and a micrometer screw in the eye-piece.

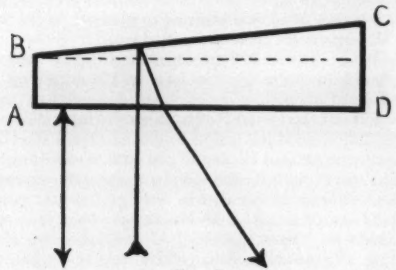


FIG. 7.

In order to ensure the cross-wires and slit being in the same plane, the slit or orifice is mounted on a plane polished glass plate, on the lower part of which the cross-wires are engraved. For high power work a disc, of which the lower half is grey, can be inserted, but in general for very accurate measurements it is preferable to use the form of eye-piece with a plane parallel disc giving a small image of a small circular source of light at the plane of the cross-wire.

In order to detect whether the images have crossed over or not, a cap is provided to rotate on the object end, which will cut off one quarter of the objective in any position, thus determining the direction of the error. It is also

proposed to make the instrument for workshop use so that prisms and parallel plates can be tested *en bloc* during the polishing process. We are indebted to Mr. R. Ives for assistance in preparing the diagrams.

Discussion.

MR. DIXEY asked Mr. Chalmers what was the smallest degree of error that could be read by the method demonstrated.

MR. CHALMERS said with very special precautions it was possible to obtain a reading which would only give an error of, approximately, two seconds, provided the surfaces were absolutely plain.

MR. DIXEY desired to know if a very small prism with a slight angle could be tested.

MR. CHALMERS replied that it could, provided the field of the telescope used was such as to give an angle three times the departure from plain surfaces.

MR. DIXEY then asked up to what angular measurement could an ordinary telescope be measured by the method demonstrated.

MR. CHALMERS replied that with an ordinary telescope nothing like a good definition would be obtained at $2\frac{1}{2}°$.

DR. DRYSDALE said he would just like to mention that Mr. Chalmers' method was simply a scientific extension of one which was always used in testing binocular prisms —to hold the prism at arm's length and observe it with the naked eye. Mr. Chalmers had also made use of the principle of auto-collimation, which was so valuable.

MR. HORACE BECK said, with regard to the degrees of accuracy of the method described, provided the glass was properly annealed, a variation could be detected as small as $\frac{1}{2}$ a second. He had seen a prism which did not give more than that amount of variation. In regard to defective annealing, he had seen a piece of glass polished parallel on two sides, and another side exactly at right angles, which had changed as much as $6''$ or $8''$ in the course of a couple of hours, due to slight difference of temperature in the room in which the glass was being worked.

THE MEASUREMENT OF ABSORPTION IN TINTED GLASSES.

PAPER BY MR. L. W. PHILLIPS, *Student Member.*

Read before the Optical Society, February 13th, 1905.

THE subject before you this evening has been, I believe, to many of you a source of interest, and also great difficulty for some time past. Those engaged in spectacle work, when using tinted glasses, have been unable to speak with certainty as to the amount of light absorbed in, or transmitted through these glasses, resource having to be taken to guess-work, the accuracy and facility of which depended upon experience. This existing need for a method of measuring absorption in tinted glasses was further emphasised by the report of the Optical Standard Committee, which was presented to you on the 21st of last April. Among the other excellent recommendations embodied in that report was one referring to coloured glasses, which read as follows:—

"That the following is a suitable series of standards for neutral tinted and coloured glasses, viz.:—

No. 1	transmitting	80%	of	incident	white	light.
„ 2	„	65%	„	„	„	„
„ 3	„	50%	„	„	„	„
„ 4	„	40%	„	„	„	„
„ 5	„	30%	„	„	„	„
„ 6	„	20%	„	„	„	„
„ 7	„	10%	„	„	„	„
„ 8	„	5%	„	„	„	„
„ 9	„	2.5%	„	„	„	„
„ 10	„	1.25%	„	„	„	„

With this recommendation all Opticians, I think, are in agreement, realising the need for such standardisation in their work. Those wishing to carry out the suggestion, however, found themselves willing but unable to do so, for they were met with the new problem of where to obtain a standard set of glasses, or if they wished to make their own tests, how to proceed. It is for the pur-

pose of trying to help you in this matter that I am bringing this paper before you for your consideration this evening.

The problem, at first sight, appears quite simple, as one has only to measure the amount of light received on a screen from a given source, and then interposing the tinted glass between the source of light and the screen, again measure the light received on the latter. The first result, minus the second, will give the amount of absorption.

As the measurement is photometric, perhaps it will be well at this point to call to your mind the general elementary principles of photometry. The method of measurement is as shown in fig. 1, and will be seen that the apparatus consists of two sources of light, the relative intensities of which are to be determined, and a

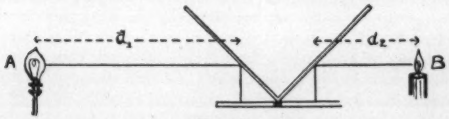


FIG. 1.

screen which is moved into such a position between the two lights that it is equally illuminated by both. If the distances between the screen and the two sources of light A and B are denoted by d_1 and d_2, respectively, then

$$\frac{A}{B} = \left(\frac{d_1}{d_2}\right)^2$$

When dealing with white lights, or lights of the same colour, the matter is very simple, but when the colours are different the matter becomes difficult. This difficulty is very similar to that experienced in trying to compare the loudness of two sounds differing in quality, such as that produced by shouting and the shrieking of a railway engine whistle. To make this colour difficulty clear to you I will have thrown on the screen two discs of white light, and you see it is very easy to tell that they are equally bright. If I now interpose two different coloured glasses in front of the apertures through which

the light is passing, you will notice that now there is difficulty in judging when the discs are of equal brightness.

THE NATIONAL PHYSICAL LABORATORY METHOD.—The measurement of absorption at the National Physical Laboratory is carried out by means of spectrophotometry; the instrument used being a modified form of the König spectrophotometer.

In this piece of apparatus light from a given source passes through two slits, one of which is covered by the glass whose absorption is to be determined. These two beams of light thus formed pass through the same train of prisms and each forms a spectrum; the two spectra being one above the other. These spectra are then polarised at right angles to one another, and any particular portion viewed through a Nicol analyser. This Nicol is then turned until the intensity of the two similar portions of the spectra are equal, and when this is the case if θ be the angle of inclination of the principal plane of the Nicol with the horizon and I_1 and I_2, the intensity of the two beams then $\frac{I_1}{I_2} = \tan^2 \theta$.

If I_2 represents the intensity of the light passing through the glass being tested, then the percentage of light transmitted $= \frac{100\ I_1}{\tan^2 \theta}$, and the absorption $= I_1 - \frac{I_1}{\tan^2 \theta} = \frac{I_1 \tan^2 \theta - I_1}{\tan^2 \theta}$.

A full description of the apparatus, with illustration and explanation of its use, is found in the paper by our President, Dr. Glazebrook, entitled, "The Optical Society and the National Physical Laboratory," and is printed in the "Transactions" for the year 1903-04. Although this is a very good method, it is only suitable for a large laboratory, as the instrument is rather costly, and as each part of the spectrum is viewed separately, the calculations for absorption are a little difficult to work out.

The necessity, therefore, was for an instrument which was simple in its construction and use, so that it should be not only inexpensive, but accurate in working, and if possible allow the measurement to be quickly carried

out. As the ordinary type of photometric screen was not of any use for the measurement of absorption in coloured glasses owing to the difficulty in making comparison, it was needful that some different method should be employed. In 1893 Professor O. N. Rood suggested the method of Flicker as a means of eliminating the colour difficulty, and it is this method, as applied in the Simmance-Abady Photometer, that I wish to speak about this evening.

Principle of the Flicker Photometer.—If we have two sources of light illuminating two different surfaces which are brought alternately into view at a suitable periodicity, when unequally illuminated, a throbbing or flicker effect will be noticed. According to one theory

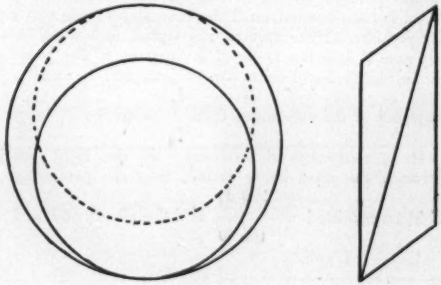


Fig. 2.

this effect is caused by the pupil of the eye failing to keep in time with the rapid change of intensity of illumination shown by the two screens. When, however, the relative positions of screens and lights are such that the surfaces viewed are equally illuminated, the intensity of the illumination seen being unchanged this flicker will disappear, and one sees, as it were, one unmoving illuminated surface. The reason for the colour difficulty being eliminated is, that the visual senses of lightness and colour are different, the former sense being much keener and quicker in action than the latter.

This view is supported by the German Optologist (Professor Hering), who states that he believes the physio-

logical senses of perception of colour and brightness to be different—the former being able to present without the latter. In a darkened room, across which a bright light flashed momentarily, although it would be possible to judge somewhat of the brightness of the beam, it would be impossible to say anything definitely as to its colour.

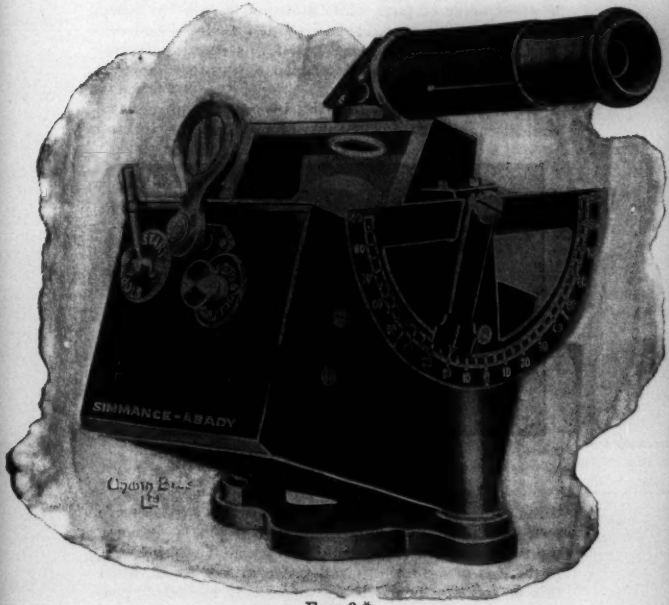


FIG. 3.*

The theory is very interesting, and I hope that those present this evening who have interested themselves in colour measurement will give their opinion on this point.

* The type of Flicker Box shown in fig. 3 can also be used for angular as well as horizontal measurements

In the Flicker Photometer, owing to the fact that the rate of bringing first one screen and then the other into view is so great, the eye has not time to take notice of the colour of the illuminated screen, but is affected only by its brightness. In the Simmance-Abady type of Flicker photometer, as shown to you this evening, it will be seen that light from the two sources is received upon a wheel made of pure white material, which, by means of a clockwork motor, is revolved at a suitable and controllable speed. The periphery of this wheel is formed by two equal conical surfaces, the appearance being as shown in fig. 2.

When the wheel revolves the intersection of the two conical surfaces crosses the line of sight, and on looking

FIG. 4.

through an eye-piece at the wheel the two surfaces are seen alternately. If the photometer is placed between the two sources of light so that they are at right angles to the line of sight and parallel with axis of wheel, when this is rotating one will see alternately the effect produced by each light. Fig. 3 shows an external view of the Simmance-Abady Photometer, while fig. 4 shows clearly the appearance of the wheel in its four positions when illuminated unequally on the opposite sides.

RELIANCE TO BE PLACED IN USE OF FLICKER PHOTOMETER.—If light from two coloured sources (say red and green) is received upon the two faces of a right-

angled wedge, balance may be obtained to a certain degree of satisfaction, but now, if the lights be moved to either double or half the previous distances from the wedge, the illumination on the two faces will no longer be found to balance. This Purkinje phenomenon, as it is known, is purely a colour effect, and, happily, does not affect the Flicker Photometer, for if it did one would have to introduce a factor dealing with the distance between the lights and the screen.

The Purkinje phenomenon is due to the fact that the retinal sense of brightness does not increase at the same rate for different colours, and is a great argument to show the necessity of entirely eliminating colour in any photometric measurement. Tests that were carried out by two gentlemen connected with the Ophthalmic

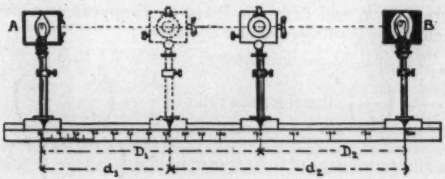


FIG. 5.

Hospital went to show that any visual defect in the eyes of the observer did not make any difference in reading. Readings were taken by observers who were respectively astigmatic, and colour blind, and another whose pupil was dilated by atropine specially for the experiment. The readings obtained by these observers were found to coincide not only when the colours were the same, but when they widely differed.

ABSORPTION MEASUREMENT.—In making the absorption measurement a bench of about 2 metres in length should be chosen, and this should be divided into millimetre divisions. If possible, a substantial type of bench should be used, but if not convenient any ordinary wooden optical bench will do. At the two ends of this bench sources of light should be arranged, and in front of these are put screens having in their centre an aperture about

three-quarters of an inch square, or, better still, if electric lamps are used they should be enclosed in a blackened box with an aperture in the front. Between these screens the photometer is put and care should be taken to arrange lamps, screens, and photometer in a straight line. On the screens a clip should be arranged for holding a spectacle glass in front of the aperture, but if not convenient at the time an elastic band will do almost as well. When the apparatus is ready for making test run the Flicker wheel at a suitable speed and move flicker box into the position where flicker disappears. If distances from lights A and B to screen are D_1 and D_2, respectively, then $\frac{A}{B} = \left(\frac{D_1}{D_2}\right)^2$

If a coloured glass is now put in front of aperture in screen at A, and flicker is found to disappear at a position where readings are now d_1 and d_2;

Then if light passing through glass be A^1 then $\frac{A^1}{B} = \left(\frac{d_1}{d_2}\right)^2$ therefore the ratio of $\frac{A^1}{A} = \left(\frac{d_1}{d_2} \times \frac{D_2}{D_1}\right)^2$

and the percentage of light transmitted $= \left(\frac{d_1}{d_2} \times \frac{D_2}{D_1}\right)^2 \times 100$

and the percentage of light absorbed will be given by the equation $100\left(1 - \left\{\frac{d_1}{d_2} \times \frac{D_2}{D_1}\right\}^2\right)$

If the length of the bench is L cms., it can easily be seen that $D_2 = L - D_1$, and $d_2 = L - d_1$.

To make this clearer, let us take a numerical example. Supposing we have a bench 200 cms. in length and balance in the two cases is at 102 cms. and 150 cms., respectively. Then D_2 and d_2 will be 98 and 50, so that percentage of light transmitted by glass tested would equal $\left(\frac{102}{98} \times \frac{50}{150}\right)^2 \times 100 = \left(\frac{51}{141}\right)^2 = 13{\cdot}08\%$

If the lights are of equal intensity D_1 will be equal to D_2, so that the percentage of light transmitted will equal

$$\left(\frac{d_1}{d_2}\right)^2 \times 100 = \left(\frac{d_1}{L - d_1}\right)^2 \times 100$$

Another method which simplifies the calculation is to have the light at B rigidly connected to the support of the photometer, so as to keep it a constant distance. If this is the case then D_2 and d_2 are equal, and percentage of light transmitted will be $\left(\frac{d_1}{D_1}\right)^2 \times 100$.

Reference to fig. 5 will make the matter clearer as to the different distances, and will also show the general arrangement of bench.

Sources of Light.—In ordinary photometry the great difficulty is to get some standard sources of light, but in absorption testing this trouble is not encountered, for the measurement is not an absolute measurement of candle-power but of the ratio between two sources of light, as will be seen by reference to calculations already given. One does not need a source of light which can always produce a definite candle-power, but rather one which can be relied on to keep constant while the tests are carried out. Even this is not necessary if the readings with the white light are taken each time between the readings with the glasses inserted in front of aperture; but if the light is constant this is avoided, and, in consequence, a good deal of time saved. In a series of tests which I carried out, the figures for which are given at the end of the paper, Nernst lamps of the old type were used.

These I found to be troublesome, as they were constantly varying in intensity of light given, very sensitive to variation of voltage, and if switched off and then on again their intensity was found to be different. If a small 6-volt lamp is used and supplied with current from a small accumulator, the voltage being constant for some time, the light given will be more steady than with a lamp run off the mains. When gas lighting is employed an Argand burner proves very satisfactory, as the small portion of centre of flame seen through aperture can be relied on for constancy. The same may be said of a burner used in conjunction with the Welsbach incandescent mantle. From the result of experiments I think the most satisfactory sources of light to use are either an Argand burner for gas, or a small glow lamp with

straight filaments supplied with current from an accumulator.

Electric lighting is to be preferred owing to its great convenience and portability in enabling lights to be quickly shifted, and also because if used in a confined space one does not have the discomfiture of the atmosphere becoming very warm, as would be the case with gas.

CONDITIONS FOR SENSITIVENESS.—Although to enable one to get sensitive results it is necessary to pay attention to several points, yet on the other hand there is nothing to deter an Optician from getting very satisfactory results with just the ordinary apparatus at his disposal.

In the first place, however, care should be taken to see that the bench is quite level, and that it is rigidly fixed. The runners on which the sources of light, screen, and photometer are fixed should be preferably of the V type, fitting without any possible shake on the central ridge of the photometer bench; a screw should also be provided to clamp them in any desired position. These supports should have, if possible, a racking mechanism to easily and accurately adjust their height. When all is ready care should be taken to see that the sources of light and photometer are in perfect alignment, otherwise it is impossible to get very good results.

It is also necessary for the flicker wheel to be kept very clean, for if it becomes dirty on either of the conical faces the flicker will not, of course, disappear. The sources of light should be capable of adjustment; if gas is used a governor being employed to carefully regulate supply, and if electric lighting an adjustable resistance should be connected in circuit. When using the photometer the speed of the wheel should be arranged for the best sensitiveness, and this is obtained by finding position one thinks balance is found and then slowing down until the speed is the lowest for which flicker will disappear. Below a certain speed the colour difficulty enters in, this minimum speed from my observation seeming to vary with the colour of the glass tested. If one finds it difficult to fix on any position of total disappearance the best method is to obtain an equal amount of flicker on both sides of where the point of disappearance should be, and then take mean reading. The most sensitive position of photometer screen is in centre of bench, therefore,

when testing a glass which passes only a very small percentage of light it is often advisable to use an intermediate tint on the other side, that is, at B, where white light would be in the ordinary method of use. This enables reading to be more near centre, and if, therefore, a small error is made it will not matter so much as if reading were at end of bench, as would have been the case if no intermediate tint had been used.

Readings should be taken as quickly as possible, consistent with accuracy, moving the photometer first in one direction and then in the other, coming back to the position where the flicker disappears. If one takes too long in settling at the position of balance the eye becomes fatigued, and cannot fix upon any definite position for disappearance. It is also better for two persons to be engaged in making observations, as this checks each reading, and at the same time gives the observer opportunity for resting the eye between the readings, thus avoiding fatigue.

Results Obtained by Ordinary Means.—The hints given above are such as to be easily compiled with, and although we should do our best it does not lie within the scope of every Optician to have everything most suitable for carrying out these measurements. No special room is necessary for making the measurements, as it will be quite satisfactory if a part of the ordinary laboratory or sight testing room with blackened walls, is divided off by means of a curtain. Even if a good bench cannot be obtained the results given below will show that satisfactory measurements can be made. The bench used in the tests taken was not very stable, and was divided into two centimetre divisions only.

Readings were taken by two observers and found to check out satisfactorily. The figures given were those obtained on several different occasions.

The results given above, I hope, will encourage those who have but little apparatus and convenience at their disposal, and show that there is good reason for them to do work of this description, even with limited apparatus and opportunities. The difference between the readings is not very much, but, with a good type of bench and sensitive arrangements, readings taken at any time will be found to be in perfect agreement.

Colour & No.		Test 1 % light passed.	Test 2 % light passed.	Colour & No.		Test 1	Test 2
Blue	1	18.4	19.25	Smoke	1	42.1	42.2
	2	41.3	43.2		2	18.4	18.2
	3	18.4	18.2		3	22.4	22.3
	4	22.1	22.8		4	54.5	53.2
	5	16.3	17.1		5	13.35	12.9
	6	16.65	16.5		6	46.5	47.5
	7	15.5	16.5		7	41.6	42.7
	8	4.1	4.15		8	18.9	18.8
	9	2.46	2.39		9	26.7	27.8
	10	43.8	43.45		10	39.8	39.5
	11	53.8	53.9		11	61.5	62
	12	57	57		12	72.2	72.8
	13	78.6	78.25		13	39.8	39.5
	14	53.5	52.5		14	7.0	7.1
	15	81.7	81.8		15	6.3	6.25
Green	1	78.5	78.6	Green	11	15.7	15.9
	2	12.2	13.1		12	13.6	13 4
	3	15.7	16.5		13	21.3	22 5
	4	19.6	20.3		14	26.5	25.3
	5	15.6	15.9		15	12.25	11.9
	6	15.1	17.1		16	10.0	11.6
	7	24.3	26.3		17	13.1	13.5
	8	15.75	15.8		18	19.1	20.6
	9	29.6	29.8				
	10	32.4	32.8				

For those, however, who simply require a standard set of tints, and do not wish to carry out any measurements themselves, it would be well if some Institution such as the National Physical Laboratory were to issue such sets so that the recommendations of the Optical Standards Sub-Committee could be carried out.

In conclusion, I should like to heartily thank Messrs. Alex. Wright and Company, the manufacturers of the Flicker Photometer, who have so kindly arranged for demonstrations this evening. I am also indebted to Messrs. Raphael, Wheway, and Redfern, for their kindness in lending a set of tinted glasses for experimental purposes.

Discussion.

Mr. DIXEY said he could have wished that he had had more opportunity of preparing a few remarks that might have been useful in connection with the subject, as there were many things he would like to have said about it. The scale of percentages that was given at the beginning of the paper, which could, no doubt, be approximated in the tints that were used for ophthalmic work, would be useful; the methods in vogue at present were not at all satisfactory. They were not scientific; being based in the old-fashioned way upon an arbitrary scale of numbers without any definite reference to the amount of light absorbed. He might say that rigid accuracy was not demanded in ophthalmic work—an approximation was all that one need ever reasonably aim at. The chief object of calibrating the tints was more for the purpose of making a prescription intelligible—that a doctor or a prescriber should convey approximately the idea of the depth of tint he wants to the man who is to make the spectacles, rather than that he himself should know the exact amount of light transmitted to his patient's eye. That must be so, because, as a matter of fact, the amount of light that is transmitted is regulated, not by the percentage of light transmitted by the glass, but by several physiological and other conditions of the sight, as for instance, the automatically acting diaphragm that they all had in front of their eyes, the iris. A certain effect of putting a dark lens in front of one's eye was to distend the iris. Then again, if Mr. Phillips was going to print this paper he would like to suggest that where he speaks of putting a spectacle lens in the diaphragm in front of the screen, he should define it as a plane lens, because if it were either a convex or a concave lens it would converge or diverge the light. If it were a convex lens it would throw more light on the photometer, or, if concave, it would act the other way. He (the speaker) did not suggest that Mr. Phillips did not know that, but it was as well to make it clear. A most interesting point referred to by him was in regard to the limit of quickness of perception—the fact that brightness is more quickly perceived than colour. There was also another point closely connected with that, and he thought it was alluded to later on in the paper—the question of the durability of time that these different senses last on the

retina. One knew perfectly well that incident light may fall with infinitesimal suddenness on the retina, yet the retina perceives it for a distinct period. That was important, because it would affect the use of the Flicker Photometer, as he understood it, from the description given, because if the rate of the revolutions were too great no flicker at all would be obtained. Light on either surface of the wheel might be different in period, and yet if the period of the revolution of the wheel was quicker than the period of sensibility of the retina to light, there was no flicker. It would be interesting to compare that periodicity with the period of the colour sense, though he did not suppose one could eliminate the light and deal only with the colour. He should be glad if Mr. Phillips could give them an estimate of the amount of light which was absorbed by a plane glass—an uncoloured plane glass as they understood it in ophthalmic work. That would, to a certain extent, affect the percentage of light transmitted by the tinted glasses. He should also like to know how the thickness of glass affected the amount of light transmitted, that was to say, if No. 3 transmits 50 per cent. with a certain thickness of glass what percentage of light would be transmitted by the same glass of exactly double the thickness?

Dr. GARNETT stated that he would like to ask a question or two. He did not learn the speed at which the cylinder revolves in order to produce the best effect in the Flicker Photometer. He did not know whether it was several revolutions per second or only a very few, and it appeared to him that very much must depend on that. If the cylinder was revolved sufficiently rapidly they would expect to get simply an average sensation whatever might be the illumination of the two sides. If they looked at an ordinary alternating arc light they saw no flicker whatever—they were not conscious of any variation of the intensity of the light, but if they looked at that light through a wheel which was attached to the alternater which generated the current, as, for example, if the current were being generated by a ring of field magnets rotating within a circle of armatures, and they peeped at the light between the gaps of the field magnets, and if, in addition, they placed their eye in a certain position, they would find that the lamp showed simply as a pair of incandescent carbons, and that the light had

gone down to a very small percentage of the ordinary illumination. If they moved their eye half the diameter of one of the field magnets, so as to catch the light at the half period, when the current was a maximum, they would see a blaze of electric light, which would appear perfectly steady between the revolving arms. If they examined the arc light with a revolving mirror they would see an effect very similar to what he had described—a great mass of flame, dying down to almost nothing, then becoming a great mass of flame again.

The lamp seemed to be perfectly steady, but the light was going through tremendous fluctuations. If their cylinder was revolved quickly enough they would get an average result, whatever the illumination on each side, and he would like Mr. Phillips to say at what speed the flicker begins when the illumination is not balanced, and then at what speed they were bound to have a flicker when the illumination is balanced in the best way possible. He was a little puzzled over another statement Mr. Phillips made, that the adjustment between the lights was not affected by any peculiarities of the eye. As far as he knew there were no two eyes which saw colours with exactly the same degree of acuteness, that is to say, the colour equation of every two people would be slightly different. The eye of one was more acute for the red, that of another more acute for the violet, or the green, as the case might be. If that was so, he could not imagine how it was that two eyes which were looking at a white light in the one case and a red light in the other, while the relative sensibility was different, arrived at exactly the same comparison. He would have expected to find the same inequalities between their adjustments as one finds in the colour equation if they endeavour to match lights with a colour box, or rotating partly-coloured disc. One might go to the extreme case: Would a person who is absolutely colour-blind adjust the instrument at the same balance as a person gifted with ordinary vision? He was very interested in the statement that the eye appreciates more rapidly variations in intensity than it does variations in colour, and he was inclined to wonder if a person were to pass very rapidly along a series of bands of colour, say red and green, what would happen. Would such a person be unconscious of any variation whatever in the light

which reached him, provided that the intensity of the green were the same as the intensity of the red? It was a very crude way of putting the question, but he thought it struck at the root of the matter—the failure to appreciate rapid variations of colour although rapid variations of intensity could be perceived.

Mr. J. C. Maxwell Garnett said there were just two points that occurred to him in listening to Mr. Phillips. The first was as to the difference between the faculty of appreciating brightness and appreciating colour. Sir William Abney, in his book on Colour Vision, states that when a light, of any colour, is diminished beyond a certain amount it is quite impossible to appreciate what colour that light is, that was to say, that if they had a very feeble green light it would look white, and a very feeble red light would also appear white. The second thing in Mr. Phillips's paper to which he wished to call attention, was the question whether the measurements tabulated at the end of the paper had been repeated with different sources of light for the source called "A" by Mr. Phillips. It seemed to him that, for instance, the proportion of light absorbed by a coloured glass would be different according as the source "A" consisted of an incandescent electric light, and of an electric arc. If, for example, it was desired to measure the amount of light absorbed by a glass with a yellow silver stain—a stain which absorbed the blue rays very powerfully, but which absorbed but little red and yellow light—and if, for the purpose of measurement, an incandescent electric light, emitting a very small proportion of rays of high refrangibility, were used as source "A," then the amount of light absorbed by the glass would be but small. If, on the other hand, an electric arc, the light from which is comparatively rich in blue rays, were used as source "A," the amount of light absorbed by the glass would be much larger. It appeared, from this example, that the proportion of light from a given source absorbed by a coloured body must depend on the nature of that source. He would, accordingly, be glad if Mr. Phillips would state whether the numbers to which he had referred had been checked against those obtained with different sources "A."

Dr. Glazebrook said there were one or two points he would like to call attention to. In the first place, he

thought Mr. Phillips gave the National Physical Laboratory rather too much credit for the method that he described at the Optical Society just about a year ago. The apparatus which he (the Chairman) described in his paper, and which he thought he showed to the meeting, was really the German König spectrophotometer. He thought Mr. Phillips's paper would lead one to suppose it was designed by themselves at the National Physical Laboratory. Of course the advantage of that apparatus for laboratory work over the one shown by Mr. Phillips—and an advantage which enables it to give an answer to the question put by Mr. Garnett—is that it is a spectrophotometer. Two spectrum colours, and not two white lights, are employed. But he quite agreed in thinking, for the ordinary comparatively rough and ready tests that are wanted, a method more like that described by Mr. Phillips was more likely to be useful; and in connection with that he would like to ask a question (which, perhaps, Mr. Dixey could answer as well as anyone). It was as to whether glasses transmitting 2.5 per cent and 1.2 per cent. are really wanted for the practical Optician's work. It was a matter that would come up again shortly, in connection with some work of his own, and if Mr. Dixey could answer that question he should be very glad.

Mr. Dixey interposing, said he could answer more certainly if he could see the glass, but he thought it quite possible that the glass was used practically as a block, and that the same purpose would be served by a bit of vulcanite. He was not quite sure. But it was the custom, though a rare one, to use these very heavy glasses.

Dr. Glazebrook, continuing, said the point, of course, really was, when one came to compare two lights which were in the ratio of 100 to 1, you were adding somewhat appreciably to the difficulties of measurement. Then, as to a question which Mr. Dixey asked, Mr. Phillips might have an answer, but it happened that they were, at the Laboratory, a few days since, comparing the absorption of two pieces of hard crown and boro silicate crown glass. He was speaking from memory, but he thought the amount of light lost in both cases was something like 8 to 9 per cent. There was really very little loss by absorption. That led him to another point he wanted to make in connection with the question, there is a loss

of about 4 per cent. from each of the two reflections that took place at the two places where the light enters and leaves the glass, so that when Mr. Phillips was dealing with the total light transmitted, the light that was lost, it must be remembered, was not all lost by absorption, but part by reflection where the light entered and where it left the lens. Mr. Phillips had a number of questions to answer and they would look forward with pleasure to his replies; but he would take this opportunity of referring to his (the speaker's) own paper about a year ago, and of reporting what progress they had made at the Laboratory with regard to carrying out some of the suggestions made in the Optical Standards Committee's report last April, to which Mr. Phillips referred. Thanks to the kindness of Mr. Aitchison, they hoped to have, quite shortly, a bench which would enable them to make some of the measurements referred to as to tests on binocular glasses, and questions of that kind. Then Mr. Stewart, of Messrs. Ross, Ltd., and Mr. Beck, had combined to equip to a very much better extent than before their apparatus for properly testing photographic lenses; and thanks to the generous anonymous donation given to him some month ago, he had been able to put in hand three benches which would enable him to make quite rapidly all the various tests, including the absorption test, that had been referred to in their discussions during the year. That was due largely to generous gifts from members of the optical trade and other gentlemen. In addition to that he hoped to be authorised next Friday to appoint an assistant, so as to take up this work for Opticians.

Mr. Phillips, in rising to reply, thanked the meeting for the kind way in which they had received his paper. Mr. Dixey had spoken of curvature; of course it should have been mentioned that the glasses tested were plane, but he was making some measurements at present to find the effect of curvature on the photometric results, so that in the near future he would be able to say more on that point. It had been mentioned that beyond a certain speed of wheel no flicker would be observed. This was not quite true, for although the range through which the flicker box could be moved without producing a flicker became greater as speed increased, yet beyond a certain point flicker again appeared.

[Mr. DIXEY here asked what was a suitable speed for wheel.]

Mr. PHILLIPS replied that a very satisfactory speed was about 300 revolutions per minute. The most sensitive speed, however, varied with the tint of glass tested. He had not the exact figures to hand at present, as measurements were still being carried out for this determination. It would be well for the flicker box to be fitted with speed regulator marked for different colours.

With regard to Dr. Garnett's remarks, he was inclined to think that there was a good deal to be said about the use with the flicker photometer of lamps supplied with alternating current. Undoubtedly there would be some little trouble if the speed of wheel was some multiple of the periodicity of the alternating current supplying the lamps used. One curious fact he had noticed that afternoon was that at a certain speed an image of the lamp filament was discernible on the flicker wheel—the lamps being supplied with alternating current. Dr. Garnett had spoken of visual peculiarities. From the different tests taken it seemed that everyone, independent of visual defects, could use the flicker photometer successfully. All that one had to do was simply to find the point at which the throbbing disappeared. Mention had already been made in the paper to some tests carried out by the designers of the photometer, Messrs. Simmance and Abady, to determine the effect of visual peculiarity.

In regard to the sense of colour, although colour vision varied with different individuals, it did not affect the use of the photometer, because colour was eliminated. What one saw was simply a disc of greyish-white tint, and the box was moved until it seemed stationary, and flicker disappeared. Mr. Garnett had spoken of the feebleness of the light—whether the intensity had any effect on the results. When working with the photometer, one would not choose too intense a source in order to prevent fatigue of the eye, and if the bench was of a short length the lights need only be of little intensity. In ordinary photometry when using a Lümmer-Brodhun, or Bunsen photometer, and dealing with coloured lights, it was the practice to take the readings with almost closed eyes to get rid of the colour as far as possible. This corre-

sponded to what Mr. Garnett had said with regard to feebleness of the sources of light.

In regard to the sources of light, A and B, they had been the same, both being Nernst lamps of the old type. He did not think different sources would affect the result owing to colour being eliminated and only the ratio of brightness compared.

Mr. GARNETT, interposing, said he had not meant having A and B different sources, but whether Mr. Phillips had employed different sources for A in the two tests, the figures for which were given? Had he taken the same measurement with different lights and tabulated two corresponding series of numbers and compared them?

Mr. PHILLIPS replied in the negative, and stated the tests were made with the same source of light at A in both cases. He had repeated the tests to show the reliance in the method and the agreement of the readings. With reference to Dr. Glazebrook's remarks he was pleased to find the trade was so loyally supporting the National Physical Laboratory, and that they were now so well-equipped for absorption measurements.

THE EARLY LITERATURE OF OPTICS.

PRESIDENTIAL ADDRESS BY PROFESSOR SILVANUS P. THOMPSON, D.Sc., B.A., F.R.S.

Delivered March 16th, 1905.

DR. GLAZEBROOK, Ladies and Gentlemen, the honour that you have accorded me in electing me as your President came upon me so suddenly and at such a busy time of the year that it was quite impossible for me to write a formal Presidential address, and I have, therefore, to offer you my apology for not being able, at the moment, to follow the precedent of all those who have sat before me in this chair. But although I am not able to offer you the formal presidential address, I take the opportunity very cheerfully of offering to you some remarks on a branch of the subject of Optics that has for long been of great interest to me, and I trust will not be without interest to you.

I propose to speak of the early literature of Optics, and I have chosen this subject as one which, although time has not permitted me to write anything about it, I have long studied. I have from time to time for many years past made the practice of diving into the early literature of many different branches of science, and I strongly recommend to all the younger men, if not to all members of the Optical Society, the practice of looking up that which has gone before in the science to which they are attached. For, as a matter of fact, all who are really interested in any scientific subject are interested not only in the subject itself, as it stands at the present day, and in its future advances, but they are really interested in knowing how that science has come to be what it is, for no sciences have come into being full fledged.

All have grown by a process of evolution, sometimes from the theoretical and sometimes from the practical side. One invention has led on to another—one mind has moved and re-acted upon another, and so the thing

has grown in many different directions. All these different ramifications that go to make up the whole of any particular science have got histories behind them. When you possess a knowledge of a science, of the facts, the observations, the laws, and the general principles, it is intensely interesting to see how the thing grew, how the facts became known, how the observations were made, how they were correlated in one another, how simple laws were observed, and then more complex, which were afterwards reduced to general principles; and to watch, ever and anon, how point after point that was found out became fruitful—not all at once, very often after a long time—fruitful in practical applications. Not the least interesting part of this study is that of observing who were the men to whom mankind, as a whole, is indebted for these various advances.

Now my reason for recommending this kind of study to the younger men, who are apt, perhaps, to think that all that is needful for them to know is to be found in the text books of the last twenty years, is this, that I earnestly hope they themselves will, in due time, become contributors to the advance of the science, either on the theoretical or the practical side, that they themselves will be among those who carry the advance forward, who will either on a small or a large scale make discoveries, observe new facts, chaïn together old facts and new ones, advance our knowledge of principles, make useful application of those already known, or discover new ones.

If they would do that, if they would take their part in the progress of the science and its applications, they may be quite sure that the process or processes by which they arrive at any advance will be of the same kind that have been fruitful in the past. No one makes an invention or discovery without some preliminary training. It may not have been in the direct line, but there must be some kind of preliminary training, and there is no surer way of learning how discoveries and inventions are to be made than to see how they were made in the past—to go back to the actual words and writings and descriptions and histories of the men who have made advances in the past.

If you want, for example, to know what Newton did, do not be content to look at the Encyclopædia Britannica

for the article on Newton; do not be content to look in the first text books of Optics as to what Newton did: go to Newton himself. Get hold of his own book, or his own published papers, his own lectures, and see exactly how he represents things. You may find at first that it is a little difficult to understand things written in an ancient phraseology—as a matter of fact, a great deal of Newton's work is written in Latin; but the line of progress that Newton had to travel, from point to point, picking up this idea and then that, trying one thing and then another, is essentially of the same kind in any science. When I say essentially, in one sense it is different, because some discoveries are made on the basis of experimental methods, and some by the application of theory. Newton was an unrivalled example of a man who could use both processes.

I recommend you, therefore, strongly, to go back as far as you can, to original treatises, to read the writings of the men themselves, and see how they got at their success—for the method of discovery, though it varies in detail, is not to be told in words. One thing has been borne in upon me in reviewing the early literature of Optics, namely, this: the practical advances did not come first, but the theoretical advances did. Advances in practice were the direct result of the thing having been thought a little onward. Theoretical advances, though seeming perfectly unfruitful, did bear fruit eventually in some kind of practical application.

Now, this early literature of Optics is provided, happily for us, with certain keys or indexes, by which we may learn where it exists. It is not in the encyclopædias, although some of them give fairly full histories of the subject. There is an admirable history of Optics, the "History and Present State of Discoveries Relating to Vision, Light, and Colours," written more than a hundred and thirty years ago, by Dr. Joseph Priestly, and if you are concerned with the subject as it existed down to the time of Priestly, there is not in the English language anything that will give you the history of the earlier phases of the science with greater precision. Besides Priestley's history there are several others, one by Dr. J. C. Fischer, a General History of Physics, published in about five volumes, in 1843. The first volume

deals with Optics before Newton, and the second, Optics in the time of Newton and his contemporaries. Another History of Optics is that of Dr. Emil Wilde, published in 1838. There is also a Bibliography of the Literature of Optics, by Professor Dove, published about 1840.

I cannot claim to have gone to any extent outside the books I have mentioned; but I should be false to the principle I have suggested to you, of going to original sources, if I had taken my history of Optics only from Priestley, Fischer, Wilde, or Dove, or from the Encyclopædias. What I have done is to try to get hold of the books themselves. I have ventured to bring down a section of my optical library, a score of books almost all anterior to the time of Newton, in order that I may tell you a few of the things to be found in this early literature of Optics.

Those who have begun to read this subject will know that the earliest reference to Optics is in the writings of the Greeks, which show that they had some very strange notions as to optical principles in general. The first reference is to be found in the writings of Pythagoras, who flourished about 600 B.C. In those early years the Greeks got some ideas of Optics, and one, which we can hardly understand nowadays, was that light or vision, or whatever you may call it, consisted in the presence of rays which went out from the eyes of people and settled upon things they were looking at. To us, who are accustomed to think of light coming from an object to the eye, the idea of the vision going out from the eye in the form of rays seems incomprehensible. I believe it to be a part of the Greek mode of thought which made the mind of man the centre of everything, and the whole universe a kind of metaphysical creation that somehow or other depended upon the individual.

Another primitive idea very hard to understand is found in Plato, who held that colour was a species of flame which played upon the surface of objects. The Greek ideas of colour were very different from ours. In the first place they did not know of the existence of as many colours as we have. They only recognised very few tints. In proof of this you have only to read Mr. Gladstone's Homeric Studies. They had not got, apparently, the same developed sense of colour that we have. They thought very simply of the matter—just as a man might

do who had not learnt the names of colours, and said that one thing looked warm and another cold, or one thing looked dark and another light. They had a very primitive colour perception. Mr. Gladstone held the view that Homer had not got so far as to understand the colour blue. I believe the theory has been advanced that man began to recognise the colours at the red end of the spectrum—that red was the first colour to be recognised by primitive man, and that yellow and green followed, and that blue was quite a late discovery. If not, then why should Homer describe the sea as being wine-coloured?

Another idea of Plato's was that light consisted of corpuscles which were shot out from luminous bodies and entered the eye. In that respect Plato differed from the earlier Greeks. Another idea was that when you saw in a mirror an image of a thing, your eye projected that image through the mirror to some point in the space at the back. That was part of the old idea of the eye sending out the rays. I have taken these few points in order that we may see how extremely primitive were the ideas of the earliest writers.

There are, then, three conflicting ideas as to the nature of light, rays shot out from the eyes to things, corpuscles which came from the things to the eye, and, lastly, the wave theory, which is quite a modern thing. Now, about the year 350 B.C., we come upon the writings of Aristotle. While Aristotle did not give us anything bearing directly upon the subject of Optics he controverted the idea of his time, that the eye shot out corpuscles or sent out rays. All things in those days were classed as belonging to one of the four elements, earth, air, water, fire. As the eye was neither earth, air, nor water, it must be a fiery body. With regard to the eye shooting out corpuscles or rays, Aristotle objected to that on the ground that if the eye was a fiery body why did we not see in the dark? He considered that objection was really sufficient to disprove the old idea. In other branches of Optics Aristotle did give us, certainly, some further advance, namely, in his book on Meteors. He treats of the rainbow, too, as having three colours, and in the same book he talks about the scintillation of the stars—why they twinkle.

It is rather surprising in a man of such gigantic

intellect, who had gone round the whole range of sciences as they existed in those days, that he did not give more attention to the science of Optics than we find in his writings.

Nearly a hundred years after Aristotle we come upon Euclid. It was known that Euclid had written a book on Optics, but it was for a long time supposed that it was lost, and I have often congratulated myself on this supposition and thought that had this not been so we might have been learning "Euclid" instead of Optics, just as, until the last year or two, the schoolboys who ought to have been learning Geometry were learning "Euclid" instead.

Euclid's Optics, however, has been found. It existed, embedded in other treatises, but we do not quite know to this day how much is Euclid's and how much is a later addition, as like many other ancient books it has been edited and added to. This book is in two parts: the first on general Optics and the second part on Catoptrics. Each part is divided into two sections, and starts off with axioms or postulates, or whatever you choose to call them, and then goes on with a number of theorems and develops the subject much in the same way as his Geometry was developed. This was about the date of 260 B.C.

A few years later we come upon that famous man, Hero of Alexandria, the inventor of water clocks, and also of the penny-in-the-slot machine. This same Hero of Alexandria dabbled in Optics. He was considering the law of reflection. You have a mirror, in front of which you have an object, and the eye—both of them, possibly, at unequal distances; and a ray passes from one to the other, being reflected at the mirror on its way. He says the particular direction taken by that ray to the mirror will be such that the total length of its path is the shortest possible distance. That states the "principle of least time" as a principle in Optics, right away back to 260 B.C.—that principle which Fermat developed afterwards and also Hamilton, and of which Mr. Chalmers has made such excellent use in our own day.

About the same time, or a little later than Hero, we have Archimedes, the man who wanted only a fulcrum and a lever, with which he said he could move the world

—the man who discovered the principle of hydrostatics, the principle of specific gravity, and the theory of displacement—by which, when a body is placed in water it causes a displacement equal to its own volume. You know the story how it is said that he found this out when he was bathing, and was so pleased that he rushed out of the bath and ran down the street crying "Eurek. Archimedes was acquainted with the law of reflexion, although he left no writings on the subject. There is a tale told of him that at the time Syracuse was besieged by the Romans, he employed the rays of the sun which he focused on the ships of the Romans and set them on fire, and thus delivered the people of Syracuse from the siege.

There exists a considerable literature on the burning mirror of Archimedes. Here is a book published in 1646, by Athanasius Kircher. This book deals with a great many things in which we are not at all interested. It treats very largely on how to make sun dials and water clocks; then there is a little bit about surveying instruments and astronomical instruments. And right at the end of the book there is something on the laws of reflexion and refraction (page 282), and it mentions the burning mirror of Archimedes. Certainly, it is not a contemporary account, as Archimedes lived about 150 B.C., and this book was published in 1646 A.D. A matter of 1,800 years difference of date is an important fact; but here is a picture of the way in which Archimedes set those ships on fire. It is not the only picture that I possess of the mirror of Archimedes. Another is to be found in a treatise on Optics by the Arabian, Alhazen, printed in Basel in 1622. It is mentioned in other treatises published in 1582, 1640, and 1730, from all of which it is clear that the mirror of Archimedes attracted a great deal of attention on the part of those who were interested in Optics. The earliest mention of it is to be found in the writings of a certain man named Anthemius, who lived about 550 A.D. Seeing that it was, therefore, some 700 years before any mention was made of it, one is justified in thinking that the burning mirror of Archimedes was an entirely spurious affair. There is no doubt that he did study Optics, and that he did also concern himself in the siege of Syracuse, and helped the defenders by inventing

catapults, and other mechanical means of defence; and someone, apparently, 700 years later or thereabouts, mixed up his optical studies with his feats on behalf of the defenders of Syracuse.

Then we come upon the writings of the Roman, Seneca, who investigated the rainbow, discovered the colours of the prism and compared them with the rainbow. Then there was Claudius Ptolemy, who lived 100 A.D. His treatise is a perfectly wonderful work. Books 1 and 2 had been lost; book 3 related to convex and flat mirrors, book 4 treated of concave mirrors; and then there was book 5, where, for the first time, we come upon a chapter on Dioptrics. Ptolemy found that the angle of refraction in water was three quarters of the angle in air.

A little earlier than Ptolemy was Pliny, in whose works there was a chapter on mirrors. The early mirrors were all of metal. The earliest mention of a mirror made of glass, silvered at the back, is in a book written in 840 A.D.

Then, coming down by a great jump from the end of the first century, and the times of Pliny and Ptolemy, we pass to the eleventh. During that thousand years, which were sometimes called the ages of faith, there was no faith in Optics; and until the Arabs reintroduced the science it was practically dead. Among the Arabs who wrote on Optics there was Alhazen, who lived about 1038 to 1100, to whom we owed the use of the magnifying glass, although, strange to say, he did not speak a word about spectacles. Then there was Roger Bacon, who lived in Oxford about 1250, and who died in 1268. He described the use of two lenses in combination, although it was doubtful if he used them as a telescope. Doubtless, he used them as a kind of magnifying glass—one above the other. Spectacles were unknown to him, but they are mentioned shortly afterwards, in 1299, by Redi, who spoke of a certain eye salve which would make an old man read more clearly—"vetri appelati occhiali" (glasses called spectacles).

Here is Porta's book on Natural Magic, dealing with many wonderful things such as the production of new planets, of changing metals, counterfeiting precious stones, the wonders of the lode-stone, of physical experiments, beautifying woman—(laughter)—of distillation,

of purifying and tempering steel, of some choice things in the art of cookery, of hunting, fowling, and fishery, of invisible writing, of burning glasses and the wonderful sights seen by them, of spectacles, whereby one could see very far beyond imagination—(laughter)—and how to see things in a chamber where things are not—(laughter). Giovanni Baptista Porta also wrote a book called "De Refractione," published in Naples in 1593. The particular copy of this work which I have brought with me, is of very exceptional interest, as it contains Porta's own manuscript notes, showing that he was getting ready for a second edition. He was the first man to notice the contraction of the pupil of the eye on going from a dark room to a light one.

Then we come to Kepler, who discovered a great part of the laws by which the planets moved. He discovered the "position" of the image, by which, he said, he meant that when you are looking at it, it was the point towards which the eyes converge. He likened this locating of the image to a sort of triangulation. He also discovered spherical aberration and proposed hyperbolic lenses to get rid of spherical aberration. He was, moreover, the first man to put a draw-tube on a telescope.

As to who invented the telescope there is no absolute certainty, but there seems to be no doubt but that it was a Dutchman. Galileo never claimed to be the inventor. He heard the thing had been done and he set to work to do it in his own way. A certain Arias Montanus, a theologian, had claimed that the telescope was known at the beginning of the Christian era, and in support of his contention he referred to the fourth chapter of Matthew and the eighth verse, and said that the kingdoms of the earth could not all have been seen at the same time unless the devil had invented the telescope. (Laughter.).

Time fails me to do more than mention the writings of Robert Boyle, a writer of many treatises, and who touched nothing that he did not adorn. Boyle tried to measure the pressure exerted by light on the surfaces of things, and he also investigated phosphorescence. Lastly, there is Robert Hooke (1635-1678), who was Secretary of the Royal Society and contemporary with Newton. He was a man who, like Boyle, went round a great many subjects, improving anything

that he touched upon. He was the first one to divide a circle accurately by using a screw to make the divisions, and he was the first man to use a dark glass on a telescope for observing the sun. His treatise, the *Micrographia*, is now very rare. This brings us to the time of Newton and Huyghens, the creators of Modern Optics; so that here we might fitly consider the earlier literature to end.

In concluding my address I hope I have interested those present in the earlier productions in Optics, and caused them to feel that there was a great deal to be learned by going to the original sources, and seeing how the science grew up to be what it was to-day. (Cheers.)

Dr. GLAZEBROOK, in moving a vote of thanks to Dr. Thompson for his address, said it had been most interesting and instructive, and was all the more valuable because it was so very distinct from what they usually listened to. To those who were struggling day by day with the practical affairs of life, it was a good thing to be carried back to those old times and to learn what was done for the subject they of the present day were studying, by those who lived so long ago. He would like to enforce what Dr. Thompson had said by a small personal recollection as to the value of original research. When he first went to Cambridge he was one day invited to the house of the Master of one of the Colleges, by whom he was asked what he was learning.

He said that he was just commencing to read Newton. The Master at once presumed he was reading it in the original, but he replied that he was reading Dr. Frosto's edition. He had a severe rating for what the Master described as the wickedness of trying to read Newton in anything but the original. Later in life, Mr. Leslie Stephen invited him to write on Newton for the Dictionary of National Biography, he thought he knew something about it and set aside a Christmas vacation to do the work in. By the end of the vacation he had on his table a pile of books dealing with Newton, which far exceeded in number those which Dr. Thompson had just been showing, but he had not written a single note. Term came and the matter had to be dropped till the summer vacation. He found that in order to make a thorough study of what men had done in the past it was necessary to go to the original works. He had

great pleasure in asking those present to offer a hearty vote of thanks to Professor Thompson for the very interesting address he had given them.

Dr. Walmsley said he should like to have the honour and the pleasure of seconding that motion. To him it had been a delightful thing to listen to Dr. Thompson's address and to hold in his hand such valuable relics of the past. It was certainly a good thing to pass from the strife and strenuousness and turmoil of the present day back into those old days when discoveries were separated by centuries, and when those discoveries were, in some instances, due to the imaginations of subsequent writers and not to the people to whom they were ascribed. He had the greatest pleasure in asking them to testify their appreciation of the intellectual treat from their new President by passing a hearty vote of thanks.

The vote was carried with acclamation.

Professor Thompson, in acknowledging, said he was glad he had succeeded in interesting them, and he hoped he had even done more than that, that, indeed, he had induced them to read for themselves those earlier portions of the literature of Optics. If so, he was quite sure they would find many other things that he had not touched upon, of very, very great interest, and things which, if they did not bear fruit immediately, would remain with them as useful mental property to come into use in due time. (Cheers.)

Mr. Aitchison said he should like to say a few words before the meeting closed, in explanation of the instrument which was on the table. The subject of the testing of binoculars had in recent years attracted considerable attention, and during the presidency of Dr. Glazebrook attention had been called to the work of the National Physical Laboratory, in the testing of optical instruments. Up to the present time they were able to get Government certificates with regard to many things, but there seemed to have been no standard with regard to testing one of the most important things in daily use—the binocular field glass.

When he (Mr. Aitchison) started the manufacture of field glasses he was not satisfied with the methods then in vogue, and he attempted to devise something better. With the assistance of Dr. Drysdale and the head of his factory (Mr. Kendall) an instrument had been devised,

and that which he was now showing was a distinct improvement on the one they had previously been using in their workshops, and he (Mr. Aitchison) believed that in the future they would be able to get even better results. However, this instrument before them would, he thought, be found of considerable interest and a starting point for some set of standards by which they would be able to get some reliable estimate as to the value of field glasses.

Dr. C. V. Drysdale said the object of the instrument was for the particular purpose of ascertaining in a binocular glass when the axes of the glass were parallel. They would, doubtless, be aware that, recently, attention had been called to the considerable strain on the ocular muscles liable to be caused if the two axes of the glass were not parallel. Furthermore, in the case of a prism glass they would be aware of the fact that unless the two prisms which effected the inversion of the image inside the glass were strictly perpendicular the result would be that the image would be slightly turned round from the true vertical position. Therefore, the arrangement before them was devised with the object of enabling those two errors to be observed simultaneously and corrected.

The first portion of the apparatus consisted simply of a projection lantern, throwing a cross like a lantern slide, upon a small screen. In order, however, to focus the image on the screen, a large lens was mounted in front of the screen, distant from it by its focal length, so that when the light coming out from the projecting tube was parallel, the image was, of course, brought to a focus on the screen. By having two similar tubes, in each of which there was a very intense source of light, such as obtained by a Nernst lamp, the two images were superimposed, and if a binocular in perfect adjustment was placed in the path of the rays the image would remain single on the screen.

But if there was any want of parallelism the cross would appear double, and a movement of two centimeters of the cross on the screen was equivalent to a want of parallelism of one degree in the binocular.

Dr. Glazebrook said he wished to express the great interest he took in the instrument, and his gratitude to Mr. Aitchison in presenting the whole outfit to the

National Physical Laboratory in order that they might be in a position to test binoculars in a satisfactory manner.

Mr. Horace Beck said he was extremely interested in the instrument which appeared to him to be a most ingenious device, and he thought it ought to do all that was claimed for it.

The President asked the meeting to return a hearty vote of thanks to Mr. Aitchison and to Dr. Drysdale for his exposition of the instrument. The Society would be very glad, he was sure, to hear that Mr. Aitchison had been so good as to put the invention to national uses by offering it to the National Physical Laboratory. They should also congratulate themselves that the apparatus had been thought out and constructed in England. The vote of thanks was accorded by acclamation, and the meeting then terminated.

SOME NOTES ON THE NATURE OF VISION.*

Paper by Mr. A. J. Bull, *Member of Council.*

Read before the Optical Society, April 17th, 1905.

I.—The Phenomena of Vision.

I am sorry to-night that I have nothing very novel to bring before you, but I hope that this subject in the form in which I propose to bring it forward may give rise to some interesting discussion. In the first place I propose to consider very briefly the chief phenomena of vision.

Firstly, we have the discrimination of light and shade, and secondly, that of colour, these being the two chief

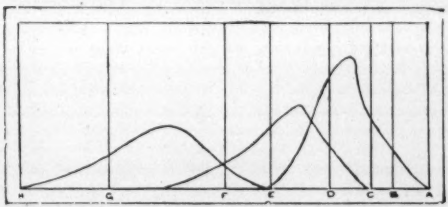


Fig. 1.—Maxwell's Colour Mixture Curves Showing the Amounts of Red, Green, and Blue Necessary to Match each part of the Spectrum.

phenomena apart from the formation of the retinal image. Then, regarding minor effects, we have the increased sensitiveness of the eye after rest. Those of us who are photographers know the dazzling effect of even subdued light after a time spent in the dark-room. Then there is the tendency of the visual effect to reach a maximum, which is seen in the failure of the eye to distinguish any variation of extremely bright lights. Fatigue is frequently observed late at night, when the eye does not see quite so well, and it is particularly noticeable in photometric work.

* Blocks for the illustrations were lent by Messrs. Longman and Company.

The effect of intermittent stimulation such as one obtains in working with rotating sectors, is interesting. I believe that the accuracy of the measurements made with them is still a matter of dispute, some experimenters having found that the eye does not correctly integrate the brightness of light when the illumination consists of flashes followed by long intervals of darkness. Mr. Jolley and I have confirmed this, and have found in the case of small angular apertures errors amounting to 20 per cent.

An interesting point is the variation of colour of a given composition with brightness. For instance, if one looks through a piece of red glass which transmits light from λ 6,000 to λ 7,000, then the apparent colour changes with the amount of light passing. When it is very bright the hue is somewhat orange, but when dull it is a deep red. I am assured by Mr. Butler that there is a distinct difference between the colour of light of the same position of the spectrum when the spectrum is produced by a grating, and when it is produced by a prism. There are slight variations of the colour sense over the surface of the retina. Then we have a most important phenomenon, viz., that all colours, except certain pure ones, can be matched by means of mixtures of three pure monochromatic lights, provided that these are suitably selected.

The after images which occur in the eye may be either bright or dark, according to the circumstances, and there is an alternation between the after images in the two eyes. That is well seen in one of Professor Bose's experiments, where two slits are placed in the stereoscope. If they are viewed together they appear as a cross, but upon watching the after images carefully you very seldom see the two together forming a cross; you see first one and then the other, and if the eyes are closed some observers very clearly see that the images alternate between an image which appears dark upon the retina, and an image which appears bright upon the retina. In fact, the retina seems to possess an intrinsic brightness even when quite at rest. There are in this case variations in the speed of the alternation with colour, producing coloured after-effects; also variations in the speed of the alternation with age, and between two eyes with temperature. If one eye is cooled or warmed, then the oscilla-

tions in that eye are retarded or accelerated, and one may then see the cross as a whole.

I may here refer to the fact that the visual impression may be quite unconscious, that the image may be received upon the retina quite apart from any consciousness of that image, and on the other hand we may have in the brain consciousness of images without any actual retinal image, giving rise to many so-called ghosts. Lastly, we may have the sensation of light produced not only by light itself, but also by blows, or by the passage of an electric current. These, in brief outline, are a few of the phenomena of vision, and before passing on I

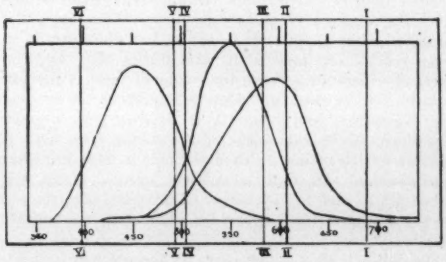


FIG. 2.—COLOUR SENSATION CURVES (ABNEY) INDICATING THE PROPORTIONS OF THE PRIMARY SENSATIONS STIMULATED BY THE VARIOUS PARTS OF THE SPECTRUM.

would like to glance at some of the theories of vision and of colour.

II.—Some Theories Relating to Vision.

Physiologists, especially in this country, appear to cling to the idea that in many physiological processes constructive and destructive changes occur in certain chemical compounds in the tissues, particularly is this applied in the case of vision, the building up and destruction of some photo-chemical substance being supposed to be part of the machinery of vision. This is well brought

out in Professor Hering's theory of colour vision. In this theory he suggests the existence of three photochemical bodies. The first of these is supposed to undergo a process of dissimulation when red light falls upon it. On the other hand green light is supposed to produce a process of assimilation. It is interesting to notice that these two colours themselves are not complementary, while the next pair are. The second chemically active body undergoes a dissimulation when yellow light falls upon it, while the opposite effect is produced by blue light. Of course, when both kinds of light act —that is, when white light falls on the retina—this theory supposes that neither of these two bodies undergo any change, but a separate substance is supposed to give the effect of light and shade.

I have not been able to find in the published accounts any actual evidence in support of theories of this class, and if any members present know of any I shall be glad to hear of it. It has been suggested, too, that a purple compound which occurs on the retina, and which is undoubtedly sensitive to the bleaching action of light, is connected with the machinery of vision, but this substance is sometimes entirely absent, so that I do not think that we can consider it essential. Then there are suggestions that the sensation of light and shade is localised in certain structures of the retina, the colour sense being given by other structures. But if the eye has a sensitiveness to any one wave length of light, then that will give the effects of light and shade whatever that wave length may be, and, further, that if it has a sensitiveness to two wave lengths somewhat separated from each other, then nothing more is required to produce colour effects, provided that the sensations produced by the two are dissimilar.

The theory of colour vision which is generally accepted among physicists is the Young-Maxwell theory. That supposes that we have in the eye three elements primarily tuned to three particular wave lengths of light, but which, by their overlapping effects, give us the continuous sensitiveness to the whole spectrum of white light. This theory carries us a certain distance, and accounts perfectly for such facts as that all compound colours can be imitated by mixtures of the three primary colours, red, green, and blue, and that the yellow pro-

duced by a mixture of pure red light and pure green light cannot be matched by pure yellow light unless this is mixed with white. But in dealing with any subject such as this, it is imperative to bear in mind the rela-

FIG. 8.—PHOTOGRAPHIC RECORDS OF ELECTRICAL RESPONSES TO MECHANICAL STIMULI.

In these experiments two similar tin wires were mounted side by side in a cell containing water and were both connected to a galvanometer. On giving one of the wires a twist through a certain angle and back to its original position, a change of electrical potential was produced, followed by a slow recovery occupying about one minute. Two galvanometer deflections and the slower recoveries are shown as recorded by a moving photographic plate. These are followed in (a) by the slower recovery following a treatment by freshly precipitated calcium oxalate. In (b) there is a complete want of recovery.

tions to it of other sciences, for fundamentally erroneous conclusions may be (and have been) arrived at if the view taken is not sufficiently broad. I, therefore, wish to review briefly some phenomena in molecular physics,

and to emphasise the analogies that have been pointed out by Professor Chunder Bose.

III.—Some Molecular Phenomena.

When energy is imparted to any substance, the whole of its physical properties are in general more or less changed, the change being dependent on the past history and consequent present state of the substance and resulting in the limit in chemical decompositon. We have instances of this constantly before us in the photographic plate, in the change undergone by copper in electric cables, in the changes undergone in Hertz-wave receivers, and so on.

In almost all cases where it has been worked out, the relation between the force applied and change produced follows a similarly shaped curve. Such is the case with the magnetisation produced in iron by magnetic forces,

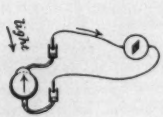


Fig. 4.—Diagram of a Freshly Excised Eye with Electrical Connections.

Made by non-polarisable electrodes to a galvanometer, which indicates the electrical changes produced by light.

the electro-motive variations that accompany a varying angle of torsion of a metallic wire and the changes produced by light in the photographic plate.

Recovery and Non-Recovery.—After the action of the force sometimes the substance recovers to its original condition, and sometimes it does not, or the recovery may be slow. The latent image of the photographic plate is a very good instance of non-recovery. By treating the plate with bichromate of potash, however, it may be made self-recovering, that is, the latent image slowly disappears.

Hertz-wave receivers, as generally used for wireless telegraphy, require tapping to bring them back to their normal resistance, but Professor Bose, by careful annealing, has obtained some that recover without such assistance. In the action of light on a silver plate in producing a change of electrical potential, the plate recovers upon stoppage of the light, although, occasion-

ally, the recovery is preceded by a sudden increase of potential, reminding one of the effect of breaking an electric circuit containing self-induction.

STAIRCASE EFFECT.—This term is applied by physiologists to the gradually increasing responses to successive equal stimuli given by an excised muscle after a period of rest. Similar effects have been found in the electric response to mechanical stimulus in the case of plants and metallic wires.

FATIGUE, taking the form of a diminishing response to successive stimuli, occurs in the electric responses of

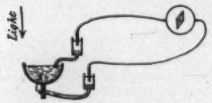


FIG. 5.—LIGHT SENSITIVE SILVER CELL WITH CONNECTIONS AS USED FOR EXCISED EYE.

excised muscles, plants, and metallic wires. Analogous effects are felt by ourselves at the beginning and end of a day's work.

REVERSAL.—This is a form of fatigue caused by continuous stimulation. It may be relative, as in the case of the photographic plate where over-exposure produces a diminished density, or it may be actual, as when the continued action of light on a silver plate reverses the direction of the electric current produced.

EFFECTS OF TEMPERATURE VARIATION.

Both heat and cold have their depressing effects upon the human body as a whole, upon the electric re-

FIG. 6.—TWO RECORDS OF THE ELECTRO-MOTIVE CHANGES PRODUCED BY LIGHT ON THE SILVER CELL.

The preliminary negative twitch is seen to precede the main deflection. The dotted line is the recovery curve.

sponse to stimulus in the excised muscle and upon the electric response to mechanical stimulus in plants. In all cases there would seem to be a temperature where a maximum effect is obtained.

The effect of a change of temperature in producing an alteration in electric response in plants depends upon the nature of the specimen, the change being small with such hardy plants as holly and ivy. The electrical changes produced by light on a silver plate are subject to similar variations with heat and cold.

ANNEALING.—In many cases annealing produces a marked effect upon the molecular state of a substance. Not only have we the well-known effects of making iron and steel more ductile and of reducing magnetic hysteresis in these same substances, but it increases and

FIG. 7.—A SERIES ELECTRICAL RESPONSES TO LIGHT, FOLLOWED BY RECOVERY DURING DARKNESS, GIVEN BY A FROG'S EYE (DR. WALLER).

The slight upward twitch upon the cessation of light may be noted in several of these responses.

makes more uniform the electric responses of metals to light and mechanical stimuli. The same effects are also produced in the resistance changes of Hertz-wave receivers.

One also sees the effects of annealing in the increased sensitiveness of a photographic plate when heated shortly before exposure. This is generally said to be due to the desiccation of the plate, but I should rather be inclined to class it with annealing phenomena. Also, the increased sensitiveness produced by heating (ripening) photographic emulsions may have some intimate relation to this phenomenon.

Effects Produced by Stimulants, Depressants and Poisons.—The stimulating and depressing effects of certain chemicals, such as alcohol, oxalic acid, potassium bromide and chloroform, find their counterpart in many molecular experiments. The stimuleting action of sodium carbonate in increasing the electric responses of wires such as tin and platinum to mechanical stimulus, and the sensitiveness of some Hertz wave receivers, is most marked.

The opposite effects of small and large doses is seen in the increase and decrease of the electric response of both plants and metals with potassium hydrate. Potassium bromide produces a depressing effect on the human system, and on electric response, which may not be unconnected with its action as a restrainer in photographic developers.

After Oscillation.—This phenomenon occurs sometimes in the recovery curves of records of the light sensitive silver cell.

The electric response phenomena to mechanical and light stimuli, referred to in this section, are the work of Professor Bose.

IV.—Visual Analogues to Molecular Phenomena.

The few representative molecular phenomena just cited are of considerable importance to us if we find some exactly corresponding phenomena in the case of vision.

Regarding the curve giving the relation of visual brightness to light stimulus, while it does not, as far as I have been able to ascertain, appear to have been worked out with any great certainty, the evidence would seem to indicate the usual characteristic of molecular changes. Certainly there exists on the one hand a minimum effective light stimulus, below which an eye fails to distinguish an image, and on the other the visual effect tends to reach a maximum, resulting in a failure to distinguish differences in very bright lights.

The variation of the hue of some colours with brightness follows at once if the curve of action is of the shape assumed. Take the case of a red light of about λ 6,000 to λ 7,000. This stimulates chiefly the red sensation in the eye, and to a much less extent the green sensation if

the light is dull. On increasing the brightness the red sensation nears its maximum first, resulting in a less increase in that sensation than in the green, which explains the change in hue from a red to an orange-red referred to before.

The time taken by any substance to recover to its normal state after stimulation, would appear to have its analogue in the persistence of vision. While the after-oscillations that sometimes follow electric response to light correspond to the oscillations of the after-images in the eye, the reversal of the electric current during the oscillation possibly being of similar nature to the black image that sometimes recurs in the retina.

Regarding the effects of intermittent illumination, unless the lag, during recovery, is equal to the lag in responding to the light stimulus, the eye will not inte-

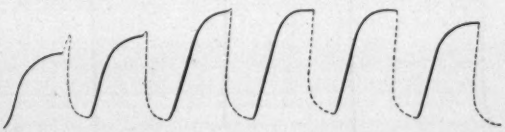


FIG. 8. – SERIES ELECTRICAL RESPONSES TO LIGHT OF A SILVER CELL. *The same upward twitch as noted with the frog's eye occurs in the first three responses.*

grate the effect correctly. As the error will vary according to the ratio of the total duration of the light to the time of recovery plus the time required to take up the full effect, it will be the more noticeable with the shorter flashes, which corresponds to the results obtained by some observers in practice.

"Staircase" and fatigue effects are to be seen in the comparative insensitiveness of the eye after heavy sleep and at the end of a long day's work, although these effects can scarcely be isolated from the state of the whole body.

Complementarily coloured after-images and contrast colours are probably connected with the fatigue of one or two of the three colour sensations.

Directly one compares a molecular phenomenon such as the electric response of a silver plate to light, to the

same phenomenon in an excised eye, then not only do we obtain the phenomenon itself, but even occasional peculiarities are repeated exactly.

With the silver plate the electric current flows from the metal to the liquid in the cell. In the eye it is nearly always from the retina to the vitreous humour. The shape of the response curve, tending to reach a maximum after a short time, is the same in both cases, as is also the tendency to attain a maximum response with successive increasing stimuli. Both responses are sometimes preceded by a slight negative twitch, and the recoveries are occasionally commenced by a positive twitch. Reversal followed by an upward recovery to-

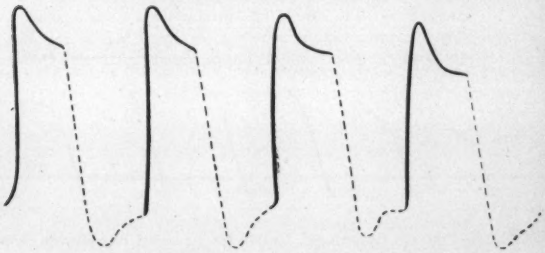


FIG. 9.—ELECTRIC RESPONSES TO LIGHT OF A SILVER CELL.

The recovery curves (dotted) during darkness, showing an after oscillation.

wards the normal potential and depression of response with increased temperature are likewise phenomena common to both.

From a consideration of all these phenomena it would appear reasonable to suppose not only (1) that we have in the retina a series of molecular receivers, tuned fundamentally to three separate frequencies of electro-magnetic radiation; and (2) that the responses of these receivers are transmitted to the brain, and there give rise to the three primary colour sensations.

But also (3) that these receivers probably act as damped receivers in responding in a lesser degree to other frequencies, thereby giving the overlapping effect which renders nearly an octave of electro-magnetic

radiation visible, and (4) that the laws which govern these receivers with regard to light stimulus and visual effect, are the same as those which govern other molecular phenomena, they are not merely analogous.

V.—Note on the Evolution of Vision.

Looking at the subject from another point of view, that of evolution, we see again that there is no reason to postulate, as is sometimes done (particularly in this country), the action of some unknowable vital force. It may be a trifle difficult at first to realise that so exquisite an organ as the eye is a direct result of the laws of evolution, but even at the present day we have in existence types of eyes, or of a sensibility to light, of a very primitive form. In the bacteria, for instance, there is a general sensitiveness diffused over the whole body. Groups of pigment cells characterise the simplest form of the light sense when localised, and in certain starfish the pigment cells are furnished with a nerve which terminates in a transparent cornea-like spot.

Then, again, the colour sense, as distinct from a perception of light and shade, is a matter of evolution, and probably arose in the insecta long before the development of the higher fruit-eating vertebrates. The power of discriminating various colours from green gave at a very early date the advantage to the insect of being able to pick out those flowers whose sweet secretions it could most easily reach, or which best suited it; while the advantage to the plant was an economisation of pollen while ensuring cross-fertilisation.

At a much later date there seems reason to believe that the colour sense in the higher vertebrates is intimately connected with origin of coloured fruits, and arose as an advantage in the struggle for food, the æsthetic perception of the beauty of colour being a thing of much later origin, at least, in man.

If, now, the sense of vision not only shows an intimate analogy to a host of molecular phenomena, but is also a product of the processes of evolution, then we cannot regard it as having had a special creation at any period of time, but we must rather consider it to be a complex manifestation of those laws of matter which act throughout all time and space.

The Chairman (Mr. Kendall), in thanking Mr. Bull for his excellent paper, said that he had given them

food for much thought and reflection. He would like to have had Mr. Bull's opinions on some of the simpler matters of vision. He (Mr. Kendall) was never satisfied with the ordinary explanation of why we saw things the right side up when the retinal image was upside down. He would also like to have Mr. Bull's opinion on the connection between the concentration of thought and accommodation, and with regard to fatigue effects in the eye in relation to colour; further, how the coloured halo effects seen in cases of incipient glaucoma were produced. Perhaps Mr. Bull would enlighten him on these points.

Discussion.

Dr. Ettles said that Mr. Bull had traversed so many interesting and important points that a very much longer time was required to adequately discuss them than could be placed at the disposal of the meeting. The whole trend of the paper was certainly such as to make one review one's ideas of animate and inanimate matter. It was, of course, purely speculative, but even abstractions were occasionally useful in causing us to review our ideas.

The lecturer expressed some doubt as to the parts played respectively by the rods and cones. The present idea was that the cones were the chief agencies in the transference of visual impulses. This idea is mainly founded on the fact that the optic disc, which consists of nerve fibres only, was insensitive to light, while the yellow spot, whose vision was far the most critical, was entirely destitute of rods. It was paved with cones only; hence, we must conclude that vision for white light and colour perception is principally maintained by the cones.

At the periphery, where the sensitiveness to colour and form diminishes, the relative proportion of rods to cones undergoes an increase. On this ground the supposition rests that the rods are primarily concerned in the light sense, an idea which is strengthened by the fact that rods alone are present in the retinæ of some nocturnal animals.

The general statement that the extreme periphery is insensitive to colour is only relatively true. So far as the low luminosity of opaque objects goes it is so, but he had been experimenting with minute coloured in-

candescent bulbs moved along the arc of a perimeter and found that the colour could be correctly estimated to the very limits of the field of vision—thus proving Landolt's statement to the same effect.

As to the part played by rhodopsin—or visual purple—a great sensation was created when Kühne discovered it. It was surmised that the photo-chemical basis of vision was made clear. Unfortunately for that view, it was next found that visual purple was absent from the yellow spot where vision is most acute. Later, however, other colouring matters, akin to visual purple, had been isolated from the cones of birds, and these are termed chromophanes. So far, they had not been satisfactorily demonstrated in the human yellow spot, but that is not tantamount to saying they are absent, for the very name "yellow" spot postulates the existence of a pigment.

A good deal of nonsense had been written on this point. Thus one author suggests that to examine visual purple, the freshly excised eye should be viewed by monochromatic (sodium) light, when it would be seen to be of a beautiful purple. There are two practical difficulties as to this. One is that in monochromatic light we are colour-blind, a purple becoming a dirty brown; the second is that sodium light will decompose it as instantly as will white light.

Such statements as this are evolved in a library not in the laboratory, and one book maker after another sticks it in his compilation without critical analysis. The result is that the science of physiological optics is a somewhat Augean stable.

Where the Hering theory of colour sensation is said to break down is this. If we gradually diminish the size of the retinal image until its area occupies a single cone, we are unable to assign to it any colour at all—whereas if the Hering theory were true the opposite should happen. Here, again, experiment did not quite bear out this objection. He had been experimenting with very distant points of light (six-inch railway signals), and as far as he could distinguish them as objects he was able to assign to them their proper colour.

The size of the retinal image could be calculated from the reduced eye of Listing, or, better still, the reduced eye of Donders. In the latter, there is a single refracting medium ($\mu = 4/3$) with a spherical surface whose

radius of curvature is — 5 mm. The principal point is at the vertex of the sphere, and the centre of curvature is at the nodal point. The first principal focus is 15 mm., and the second is — 20 mm., while the diameter of a foveal cone is taken to be .002 mm.; the phenomena of diffraction and irradiation, however, made accurate observations very difficult.

Before we theorise too much about vision we should ascertain what changes are now known to actually take place in stimulation of the retina.

1. There is bleaching of the visual purple.
2. The electro-motive force is diminished—as shown by the deflection of a galvanometer when light is admitted into a freshly excised eye.
3. The pigment layer of the retina throws processes down into the rod and cone layer.
4. Engelmann has shown that the cones undergo contraction.

The value of sufficient luminosity in perception must be constantly borne in mind. In the light sense we have two functions: firstly, that which gives us the liminal intensity or "threshold"; and, secondly, the sense of light difference. By the first is meant the amount of admixture of white with absolute black, just enough and no more, to enable us to say that light is present.

From thence onwards we pass not in arithmetical progression, but as the logarithm of the stimulus to a point where the dazzling is such that no further increase is perceptible.

By the second is meant that sense which differentiates one luminosity from another. Two equally luminous objects are regarded and the illumination of one is gradually diminished until we see a difference—that point is the measure of our sense of light difference. Practically it may be estimated by turning down the gas, illuminating a Snellen's test type until it affects our own presumably healthy eye. In many states of disease with otherwise good vision this light sense is profoundly altered so that an eye capable of reading 6/6 in a good light will fall to 6/60 under conditions when it should still see 6/6. This is a most useful point, because it always means grave disease.

Mr. Jolley, in continuing the discussion spoke of the fatigue effects that occur in photometric work, there being an oscillation in the sensitiveness of the eye to changes in luminosity with long continued series of readings. He referred to the Weber-Fechner law being now somewhat discredited by physicists.

Mr. Bull, in the course of his reply, said he was very glad to see Dr. Ettles present, as he was interested to hear any evidence as to the existence of a black and white sensation in the eye, but, as he rather anticipated, none of the evidence appeared to be very strong. Regarding the colours of very small retinal images, he thought from his own observations that although they were more difficult to recognise than those of larger images, it was still possible always to distinguish their correct colours. He had not regarded the Weber-Fechner law, as even if correct it only applied to a part of the total curve of vision.

LIGHTHOUSES.*

LECTURE BY MR. W. ROSENHAIN, B.A., B.C.E., *Member of Council.*

Delivered June 23rd, 1905.

THE lecturer dealt with the subject rather from the point of view of the lights themselves than that of the "houses" by which they are carried; he began by giving a brief outline of the purposes served by different classes of lights, dividing them into the "landfall" lights, which serve to warn the mariner of his approach to a coast from the open ocean, coast lights which mark the projecting points and hidden dangers of the coast, and harbour lights which guide the navigator in threading narrow channels. The lecturer then briefly reviewed the manner in which the requirements of these various lights had been met in the past, illustrating ancient Roman lighthouses or beacon towers by pictures obtained from medals and bas-reliefs on which lighthouses are shown. Early English lighthouses were illustrated by reproductions from the bill-heads for lightdues of the 17th century, showing coal fires on the summits of wide towers.

The next step was shown by a picture of the candle holder which was in use on the Eddystone until 1817. The lecturer then dealt with the advantages of concentrating the available light upon the sea, and showed how this was done in the first place by means of parabolic reflectors; the disadvantages of this mode of light-gathering were then pointed out, and the invention of annular lighthouse lenses by Augustine Fresnel was then described. The application of this invention to the old fixed and slowly-revolving lights was illustrated by means of photographs of actual lighthouse apparatus constructed by Messrs. Chance, and the more modern developments, involving greater concentration of light in the horizontal plane and more rapid rotation by means of the mercury float were then described, with special reference to the Hopkinson group-flashing lights which have so consider-

* This lecture, which was copiously illustrated through the Lantern, is reported in abstract only.

ably increased the number of available distinctions between neighbouring lights.

Some of the largest of modern lighthouse appliances were then described, as well as the arrangements of towers and lanterns used for their protection. Finally, the lecturer dealt with lighthouse illuminants, describing the evolution of the multiple-wick oil burner, from the simple central-draught burner of Argand, to Fresnel's mechanical lamp and the six and eight-wick burners which remained in use until quite recently; the struggle between the oil burner and its rivals, gas and electricity, was traced, and the most modern development in lighthouse illumination, the incandescent mantle heated by a flame of vapourised heavy mineral oil, was illustrated by reference to the Chance burner of that type.

Mr. Hyatt-Woolf, at the close of the lecture, said it was his pleasing duty to propose a vote of thanks to Mr. Rosenhain. He could not invite discussion, because it was inappropriate to a lecture. He thought the ladies and gentlemen present owed a great debt of gratitude to the lecturer for having taken them with him away from this dreary city on a tour of inspection throughout the ocean-ways of the world, to inspect those ingenious light devices that made for the guidance and safety of ocean-voyagers. He felt that in view of the season and the fact that so many present would shortly be on the ocean, the lecture was particularly appropriate.

Dr. Mullineux Walmsley seconded the vote of thanks, and expressed his own appreciation of the delightful lecture. He agreed with Mr. Hyatt-Woolf that the optical details of lighthouses had apparently been carried to such perfection that but little improvement seemed possible.

The vote of thanks to Mr. Rosenhain was carried with acclamation.